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The **Review Index** (pp. 57–80) lists all books alphabetically by their authors, *not* by the authors of the reviews. The year of publication following an author's name is that of the edition being reviewed. The book title is followed by the reviewer's name, and the year of publication, volume and page numbers of the review.

The **Subject Index** (pp. 2–28) covers articles only. Under each heading, reference is made to the author, year of publication and title of each article dealing substantially with that subject; the full reference is then given under that entry in the Author Index. Subjects are not listed under alternative headings, and any subject not found under one heading should be looked for under vaguely synonymous headings. Since selection of headings, and of articles to go under them, is notoriously a disputable matter, it should be noted that the Editor is entirely to blame for all such defects. For the more important and tedious job of compiling, checking, typing, setting and proof-reading the whole index, the *Journal* is much indebted to Marguerite Morris, Verna Cole, Kit and Roger Buchdahl, Nick Darlington, Robin Little and the Aberdeen University Press.

D. H. MELLOR

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Whyte, L. L. (1954) A dimensionless physics?

Discovery
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Ellis, G. F. R. (1966) **R** Törnebohm, H. (1963)

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Emmet, D. (1968) **R** Schmidt, P. F. (1967)

Evans, J. D. G. (1968) **R** Regnéll, H. (1967)

Ezriel, H. (1956) Experimentation within the psycho-analytic session. **7,** 29–48. **D** Spilsbury, R. J. (1957) **7,** 338–42; Ezriel, H. (1957) **7,** 342–7

Ezriel, H. (1957) **N** Spilsbury, R. J. (1957)

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Fairbairn, W. R. D. (1957) **N** Balint, M. (1957)

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Feyerabend, P. K. (1956) **R** Martin, G. (1955)

Feyerabend, P. K. (1957) **R** Landé, A. (1955)

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Feyerabend, P. K. (1961) **R** Harré, R. (1960)

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Feyerabend, P. K. (1967) **R** Reichenbach, H. (1965)

Feyerabend, P. K. (1968) A note on two 'problems' of induction. **19,** 251–3 **D** Foster, L. (1969) **20,** 259–60; Settle, T. W. (1969) **20,** 352–5

Fine, A. I. (1964) **N** Goodstein, R. L. (1963)

Fine, A. I. (1968) **R** Feyerabend, P. K. and Maxwell, G., Eds. (1966)

Fine, A. I. (1968) **R** Feynman, R. (1967)

Fine, A. I. (1969) **R** Humphreys, W. C. (1968)

Fisk, M. (1967) A defence of the principle of event causality. **18,** 89–108

Flew, A. (1951) **R** Cornforth, M. (1950)

Flew, A. (1952) **R** Tran-Duc-Thao (1951)

Forrest, D. W. (1962) **R** Buytendijk, F. J. J. (1961)

Foster, L. (1969) **N** Feyerabend, P. K. (1968)

Foulkes, S. H. (1957) Comments on Fairbairn's paper. **7,** 324–9. **D** Fairbairn, W. R. D. (1957) **7,** 333–8

Fozzy, P. J. (1963) **N** Mackay, D. M. (1962)

Frank, P. (1950) Metaphysical interpretations of science. **1,** 60–74; 77–91

Frankfurt, H. G. and Poole, B. (1966) **N** Canfield, J. (1964)

Freed, L. (1953) Dualism and language. **3,** 327–38

Fröhlich, H. (1969) **R** Bunge, M., Ed. (1967)

Gabor, D. (1957) **R** Whyte, L. L. (1955)

Gabor, D. (1958) **R** Ashby, W. R. (1956)

Gabor, D. (1966) **R** Couffignal, M. L., Ed. (1965)

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Gardiner, P. (1967) **R** Danto, A. C. (1965)

Gauld, A. (1966) Could a machine perceive? **17,** 44–58. **D** Jones, B. (1969) **20,** 261–2

Geach, P. T. (1965) **N** King-Farlow, J. and Hall, E. A. (1965)

Geach, P. T. (1969) **R** De Mauro, T. (1967)

Gellner, E. (1957) **R** Edel, A. (1955)

Gellner, E. (1961) **R** Merton, R. K. (1957)

George, P. (1952) **R** Forbes, R. J. (1948)

George, P. (1953) **R** Forbes, R. J. (1950)

Gibson, L. (1969) **N** Hooker, C. A. and Stove, D. (1968); Mackie, J. L. (1963)

Giedymin, J. (1963) Reliability of informants. **13,** 287–302

Gillies, D. A. (1968) **R** Nielsen, H. A. (1967)

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Goldstein, L. J. (1958) The two theses of methodological individualism. **9,** 1–11 **D** Watkins, J. W. N. (1959) **9,** 319–20; Goldstein, L. J. (1959) **10,** 240–1; Watkins, J. W. N. (1959) **10,** 242–4

Goldstein, L. J. (1959) **N** Watkins, J. W. N. (1959)

Gombrich, E. H. (1954) **R** Ivins, W. M. (1953)

Good, I. J. (1958) **R** Brown, G. S. (1957)

Good, I. J. (1959) A theory of causality. **9,** 307–10

Good, I. J. (1959) **N** Abramenko, B. (1958)

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Good, I. J. (1961) A causal calculus. **11,** 305–18; **12,** 43–51

Good, I. J. (1961) **N** Good, I. J. (1960)

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Good, I. J. (1967) On the principle of total evidence. **17,** 319–21

Good, I. J. (1967) The white shoe is a red herring. **17,** 322. **D** Hempel, C. G. (1967) **18,** 239–40; Good, I. J. (1968) **19,** 156–7

Good, I. J. (1967) Human and machine logic. **18,** 144–7. **D** Lucas, J. R. (1968), 155–6; Good, I. J. (1969) **19,** 357–8

Good, I. J. (1968) Corroboration, explanation, evolving probability, simplicity and a sharpened razor, **19,** 123–43

Good, I. J. (1968) **N** Good, I. J. (1967)

Good, I. J. (1969) **N** Good, I. J. (1967)

Gooddy, W. (1960) **R** Birkenhead, Lord (1958)

Goodstein, R. L. (1952) Mathematical logic. **R** Curry, H. B. (1951) and Robinson, A. (1951)

Goodstein, R. L. (1960) **R** Nagel, E. and Newman, J. R. (1958)

Goodstein, R. L. (1963) The significance of incompleteness theorems. **14,** 208–20. **D** Fine, A. I. (1964) **15,** 140–1; Goodstein, R. L. (1964) **15,** 141

Goudge, T. A. (1958) Causal explanations in natural history. **9,** 194–202

Gould, R. P. (1957) The place of historical statements in biology. **8,** 192–210

Gould, R. P. (1960) **R** Darlington, C. D. (1959)

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Grant, C. K. (1955) **N** Scriven, M. (1954)

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Gregory, R. L. (1953) On physical model explanations in psychology. **4,** 192–7

Grene, M. (1958) Two evolutionary theories. **9,** 110–27; 185–93. **D** Bock, W. J. and von Wahlert, G. (1963) **14,** 140–6; Grene, M. (1963) **14,** 152–4; Carter, G. S. (1964) **14,** 345–9; Grene, M. (1964) **14,** 349–51

Grene, M. (1961) Statistics and selection. **12,** 25–42

Grene, M. (1962) **R** Waddington, C. H. (1960)

Grene, M. (1962) **R** Thorpe, W. H. (1960)

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Grice, G. R. (1968) **R** Hartman, R. S. (1967)

Gruender, C. D. (1966) The Achilles paradox and transfinite numbers. **17,** 219–31

Grünbaum, A. (1953) Whitehead's method of extensive abstraction. **4,** 215–26

Grünbaum, A. (1954) **N** Blair, G. W. S. (1953). **D** Whitrow, G. J. (1954) **5,** 151

Grünbaum, A. (1956) **N** Roshwald, M. (1955) **D** Roshwald, M. (1956) **7,** 240–2

Grünbaum, A. (1959) The falsifiability of the Lorentz-Fitzgerald contraction hypothesis. **10,** 48–50. **D** Popper, K. R. (1959) **10,** 50; Dingle, H. (1959) **10,** 228–9; Keswani, G. H. (1960) **11,** 50–5; Grünbaum, A. (1960) **11,** 143–5

Grünbaum, A. (1960) **N** Dingle, H. (1959). **D** Dingle, H. (1960) **11,** 145;

Grünbaum, A. (1961) **12,** 153–6; Dingle, H. (1961) **12,** 156–7

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Grünbaum, A. (1969) Can an infinitude of operations be performed in a finite time? **20,** 203–18

Grünbaum, A. (1969) **N** Dobbs, H. A. C. (1969)

Gunderson, K. (1968) Robots, consciousness, and programmed behaviour. **19,** 109–22

Guthrie, W. K. C. (1953) **R** van Melsen, A. G. (1952)

Hacker, P. M. S. (1967) **R** Bennett, J. (1964)

Hacking, I. (1964) On the foundations of statistics. **15,** 1–26

Hacking, I. (1965) **R** Katz, J. J. (1962)

Hacking, I. (1965) **R** Kyburg, H. E. and Nagel, E., Eds. (1963)

Hacking, I. (1966) Subjective probability. **R** Kyburg Jr., H. E. and Smokler, E. H., Eds. (1964)

Haldane, J. B. S. (1951) **R** Sommerhof, G. (1950)

Haldane, J. B. S. (1952) **R** Young, J. Z. (1951)

Haldane, J. B. S. (1952) **N** Ashby, W. R. (1952). **D** Hick, W. E. (1953) **3,** 353–5

Haldane, J. B. S. (1955) **N** Woodger, J. H. (1953). **D** Woodger, J. H. (1956) **7,** 149–55

Hall, E. A. and King-Farlow, J. (1965) Man, beast and philosophical psychology. **16,** 81–101. **D** Geach, P. (1965) **16,** 248

Hall, P. G. (1962) **R** Bunbury, E. H. (1959)

Hall, R. (1967) **R** Brentano, F. (1966)

Halsbury (1955) **N** Bondi, H. (1955). **D** Bondi, H. (1955) **6,** 244

Halsey, A. H. (1961) **R** Sorokin, P. A. (1958)

Hamblin, C. L. (1965) **R** Alexander, P. (1963)

Hamlyn, D. W. (1967) **R** Shwayder, D. S. (1965)

Hamlyn, D. W. (1968) **R** Direnzo, G. J., Ed. (1966)

Hanson, N. R. (1961) Discovering the positron. **12,** 194–214; 299–313. **D** Hutten, E. H. (1962) **13,** 54–5; Hanson, N. R. (1962) **13,** 55

Hanson, N. R. (1962). **N** Hutten, E. H. (1962)

Harper, W. L. and Kyburg, H. E. (1968) The Jones case. **19,** 247–51. **D** Levi, I. (1969) **20,** 153–9

Harrah, D. (1959) **N** Gallie, W. B. (1957). **D** Gallie, W. B. (1959) **10,** 239–40

Harris, E. E. (1968) Simultaneity and the future. **19,** 254–6

Harris, F. T. C. (1961) **R** Cannon, H. G. (1958)

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Harré, R. (1961) **R** Lerner, D. Ed. (1959)

Harré, R. (1962) Philosophical aspects of cosmology. **13,** 104–19

Harré, R. (1963) **R** Körner, S. (1960)

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Hayek, F. A. (1955) Degrees of explanation. **6,** 209–25

Heathcote, A. W. (1954) William Whewell's philosophy of science. **4,** 302–14

van Heerden, P. J. (1961) A point of Professor Dingle's. **12,** 70. **D** Dingle H. (1961) **12,** 71

Hempel, C. G. (1967) **N** Good, I. J. (1967)

Henry, D. P. (1966) **R** Tymieniecka, A. and Parsons C., Ed. (1965)

Hesse, M. B. (1952) Operational definition and analogy in physical theories. **2,** 281–94

Hesse, M. B. (1953) Models in physics. **4,** 198–214

Hesse, M. B. (1953) **R** Milne, E. A. (1952)

Hesse, M. B. (1953) **R** Boole, G. (1952)

Hesse, M. B. (1956) **R** Boirel, R. (1955)

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Hesse, M. B. (1958) **9,** 128–9

Hesse, M. B. (1958) **N** Hesse, M. B. (1958)

Hesse, M. B. (1958) **R** Nagel, E. (1956)

Hesse, M. B. (1959) **R** Jammer, M. (1957)

Hesse, M. B. (1959) **R** Toulmin, S. *et al.* (1957); Foster, M. B. (1957)

Hesse, M. B. (1960) Gilbert and the historians. **11,** 1–10; 130–42

Hesse, M. B. (1960) **R** Barker, S. F. (1957)

Hesse, M. B. (1960) **R** Bunge, M. (1959)

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Hesse, M. B. (1967) **R** Hempel, C. G. (1965)

Hesse, M. B. (1967) **R** Kargon, R. H. (1966)

Hesse, M. B. (1968) **R** Feyerabend, P. K. and Maxwell, G., Ed. (1966)

Hesse, M. B. (1968) **R** Scheffler, I. (1967)

Hesse, M. B. (1969) Ramifications of 'Grue'. **20,** 13–25. **D** Hunt, G. M. K. (1969) **20,** 257–9

Hesse, M. B. (1969) **R** Edwards, P. (1967)

Hick, W. E. (1953) **N** Ashby, W. R. and Haldane, J. B. S. (1952)

Hinde, R. A. (1956) Ethological models and the concept of 'drive'. **6,** 321–31
Hirschmann, D. (1966) **R** Baumrin, B., Ed. (1963)
Hirschmann, D. (1967) **R** Caws, P. (1965)
Hirst, R. J. (1962) **R** Hamlyn, D. W. (1961)
Hirst, R. J. (1963) **R** Yolton, J. W. (1962)
Hirst, R. J. (1965) **R** Smart, J. J. C. (1963)
Hoering, W. (1969) Indeterminism in classical physics. **20,** 247–55
Hooker, C. A. and Stove, D. (1968) Relevance and the ravens. **18,** 305–15. **D** Gibson, L. (1969) **20,** 75–80
Horowitz, I. L. (1965) In defence of the sociology of knowledge. **16,** 239–41. **D** Jarvie, I. C. (1965) **16,** 241–2
Hotopf, W. H. N. (1963) **R** Thomson, R. (1959)
Howard, I. P. (1958) **R** Brunswik, E. (1956)
Howson, C. (1969) **R** Kyburg Jr., H. E. (1968)
Hull, D. L. (1965) The effect of essentialism on taxonomy—two thousand years of stasis. **15,** 314–26; **16,** 1–18
Hunt, G. M. K. (1969) **N** Hesse, M. B. (1969)
Hunt, G. M. K. (1969) **R** Theobald, D. W. (1968)
Hutchison, T. W. (1953) Berkeley's *Querist* and its place in the economic thought of the eighteenth century. **4,** 52–77
Hutten, E. H. (1951) **R** Joad, C. E. M. (1950)
Hutten, E. H. (1951) **R** Margenau, H. (1950)
Hutten, E. H. (1951) On the principle of action by contact. **2,** 45–51
Hutten, E. H. (1952) **R** Whittaker, E. (1949)
Hutten, E. H. (1952) **R** Bridgman, P. W. (1950)
Hutten, E. H. (1952) **R** Joergensen, J. (1951)
Hutten, E. H. (1953) **R** Weyl, H. (1951)
Hutten, E. H. (1953) **R** Lensky, L. Ed. (1952)
Hutten, E. H. (1953) **R** Wittgenstein, L. (1953)
Hutten, E. H. (1954) The role of models in physics. **4,** 284–301
Hutten, E. H. (1954) **R** Price, H. H. (1953)
Hutten, E. H. (1954) **R** de Broglie, L. (1953)
Hutten, E. H. (1954) **R** Strawson, P. F. (1952)
Hutten, E. H. (1954) **R** Destouches, J-L and de Broglie, L., Eds. (1953)
Hutten, E. H. (1955) **R** Törnebohm, H. (1952)
Hutten, E. H. (1955) **R** Cassirer, E. (1953)
Hutten, E. H. (1955) Note on 'The age of the universe'. **6,** 58–61
Hutten, E. H. (1955) **R** Frenkel-Brunswik, E. (1954)
Hutten, E. H. (1955) **R** Kantor, J. R. (1953)
Hutten, E. H. (1956) On explanation in psychology and in physics. **7,** 73–85
Hutten, E. H. (1956) **R** Freud, S. (1954); Jones, E. (1953–5)
Hutten, E. H. (1958) Probability and induction. **R** Fisher, R. A. (1956)
Hutten, E. H. (1958) **R** Sambursky, S. (1956)
Hutten, E. H. (1959) **R** Jones, E. (1957)
Hutten, E. H. (1959) **R** Février, P. (1956)
Hutten, E. H. (1960) **R** Sambursky, S. (1959)
Hutten, E. H. (1960) **R** Wiener, P. P. and Noland, A., Eds. (1957)
Hutten, E. H. (1960) **R** Stokes, A. (1958)

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Hutten, E. H. (1960) **R** Feigl, H. *et al.* (1958)
Hutten, E. H. (1962) **N** Hanson, N. R. (1961). **D** Hanson, N. R. (1962) **13,** 55
Hutten, E. H. (1963) **R** Sambursky, S. (1962)
Hutten, E. H. (1963) **R** Solmsen, F. (1960)
Hutten, E. H. (1963) **R** IUHPS (1961)
Hutten, E. H. (1965) **R** Gershenson, D. E. and Greenberg, D. A. (1964)
Hutten, J. M. (1960) **R** Williams-Ellis, A. (1958)
Jacob, E. F. (1953) **R** Crombie, A. C. (1952)
Jammer, M. (1963) The factorisation of energy. **R** Hiebert, E. N. (1962)
Jardine, N. (1967) The concept of homology in biology. **18,** 125–39
Jardine, N. (1967) **R** Harré, R. (1965)
Jardine, N. (1969) **R** Needham, J. (1968)
Jarvie, I. C. (1961) Nadel on the aims and methods of social anthropology. **12,** 1–24
Jarvie, I. C. (1961) **R** Winch, P. (1958)
Jarvie, I. C. (1961) **R** Gibson, Q. (1960)
Jarvie, I. C. (1963) **R** Pocock, D. F. (1961)
Jarvie, I. C. (1964) **R** Horowitz, I. L. (1961)
Jarvie, I. C. (1964) **R** Passmore, J. (1961)
Jarvie, I. C. (1964) **R** Schoek, H. and Wiggins, J. W., Eds. (1960); Schoek, H. and Wiggins, J. W., Eds. (1961)
Jarvie, I. C. (1964) Explanation in social science. **R** Brown, R. (1963)
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Jarvie, I. C. (1965) **N** Horowitz, I. L. (1965)
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Jeffreys, H. (1954) **R** Braithwaite, R. B. (1953)
Jeffreys, H. (1955) The present position in probability theory. **5,** 275–89
Jeffreys, H. (1956) **R** Dingle, H. (1954)
Jones, B. (1969) **N** Gauld, A. (1966)
Jones, P. (1966) **R** Bartley III, W. W. (1964)
Jones, P. (1969) **R** Lazerowitz, M. (1968)
Kahane, H. (1967) **N** Baumer, W. H. (1964)
Kahane, H. (1969) **N** Baumer, W. (1964)
Kapp, R. O. (1953) **R** Ashby, W. R. (1952)
Kapp, R. O. (1954) Living and lifeless machines. **5,** 91–103
Kapp, R. O. (1955) A new interpretation of gravitation. **5,** 331–2
Kapp, R. O. (1955) **R** Coulson, C. A. (1955)
Kapp, R. O. (1955) Hypotheses about the origin and disappearance of matter. **6,** 177–85. **D** Bondi, H. (1955) **6,** 239–41; Kapp, R. O. (1955) **6,** 241–3; Halsbury, (1955) **6,** 243–4; Bondi, H. (1955) **6,** 244
Kapp, R. O. (1955) **N** Bondi, H. (1955)
Kapp, R. O. (1956) **R** Meurers, J. (1954)
Kapp, R. O. (1956) **R** Glover, E. (1956)
Kapp, R. O. (1957) **R** Woodger, J. H. (1956)
Kapp, R. O. (1957) **R** Broda, E. (1955)
Kapp, R. O. (1957) **N** Dobbs, H. A. C. (1957). **D** Dobbs, H. A. C. (1958) **8,** 306–9

Kapp, R. O. (1958) Ockam's razor and the unification of physical science. **8,** 265–80. **D** Schlesinger, G. (1960) **11,** 55–9

Kapp, R. O. (1959) Space. **10,** 1–15

Kapp, R. O. (1960) **N** Schlesinger, G. (1960)

Kapp, R. O. (1961) **N** Schlesinger, G. (1961)

Kapp, R. O. (1965) The logic and psychology of science. **R** Hutten, E. (1962)

Keenan, C. W. (1966) **R** Benjamin, A. C. (1965)

Keswani, G. H. (1960) **N** Popper, K. R. and Grünbaum, A. (1959). **D** Born, M. (1961) **12,** 150–1; Levinson, H. B. (1965) **16,** 246–8

Keswani, G. H. (1961) **N** Born, M. (1961). **D** Levinson, H. B. (1965) **16,** 246–8

Keswani, G. H. (1965) Origin and concept of relativity. **15,** 286–306; **16,** 19–32; 273–94. **D** Dingle, H. (1965) **16,** 242–6; Keswani, G. H. (1966) **17,** 149–52

Keswani, G. H. (1966) **N** Keswani, G. H. (1965)

Keswani, G. H. (1966) **N** Popper, K. R. (1966)

Kilmister, C. W. and Bondi, H. (1959) The impact of *Logik der Forschung*. **R** Popper, K. R. (1959)

Kilmister, C. W. (1962) **R** de Beauregard, O. C. (1957)

King-Farlow, J. and Hall, E. A. (1965) Man, beast, and philosophical psychology. **16,** 81–101. **D** Geach, P. (1965) **16,** 248

Klappholz, K. (1964) Value-judgments and economics. **15,** 97–114

Klappholz, K. (1969) **R** Shackle, G. L. S. (1966)

Klein, M. J. (1953) Order, organization and entropy. **4,** 158–60. **D** Burgers, J. M. (1954) **5,** 70–1

Kneale, W. (1954) **R** Boehner, P. (1952)

Kneale, W. (1957) **N** Bennett, J. (1956–7)

Kneale, W. (1957) **R** Wilson, C. (1956)

Kneale, W. (1960) **R** Jevons, W. S. (1958)

Kneale, W. (1961) Universality and necessity. **12,** 89–102

Kneale, W. (1962) **R** Weisheipl, J. A. (1960)

Kneale, W. (1963) **R** Venn, J. (1962)

Kneale, W. (1963) **R** Pap, A. (1962)

Kneale, W. (1965) **R** Jevons, W. S. (1958)

Kneale, W. (1966) **R** Gregg, J. R. and Harris, F. T. C., Eds. (1964)

Kneale, W. (1967) **R** Lloyd, G. E. R. (1966)

Kneale, W. (1968) **R** Carnap, R. (1967)

Kneale, W. (1968) Scientific revolution for ever? **19,** 27–42

Kneebone, G. T. (1965) **R** Kneale, W. and Kneale, M. (1962)

Körner, S. (1959) **R** Born, M. (1956)

Körner, S. (1959) **R** Russell, B. (1958)

Körner, S. (1959) **R** Klibansky, R., Ed. (1958)

Körner, S. (1964) Deductive unification and idealisation. **14,** 274–84

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Körner, S. (1968) **N** Kumar, D. (1967)

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Kreisel, G. (1953) The diagonal method in formalised arithmetic. **R** Mostowski, A. (1952)

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KRISHNA, D. (1961) 'Lying' and the compleat robot. **12,** 146–9

KUHN, T. S. (1966) **R** AGASSI, J. (1963)

KUHN, T. S. (1967) **R** WILLIAMS, L. P. (1965). **D** WILLIAMS, L. P. (1967) **18,** 230–3

KUMAR, D. (1967) Logic and inexact predicates. **18,** 211–22. **D** KÖRNER, S. (1967) **18,** 323–4; KUMAR, D. (1969) **19,** 353–8

KUMAR, D. (1969) **N** KUMAR, D. (1967)

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KYBURG Jr., H. E. and HARPER, W. L. (1968). The Jones case. **19,** 247–51. **D** LEVI, I. (1969) **20,** 153–9

LACEY, H. M. (1969) **R** GOLD, T. and SCHUMACHER, D. L., Eds. (1967)

LAKATOS, I. (1963) Proofs and refutations. **14,** 1–25; 120–39; 221–45, 296–342

LAMONT, C. (1962) **R** DUCASSE, C. J. (1961)

LANDÉ, A. (1956) The logic of quanta. **6,** 300–20

LANDÉ, A. (1957) **R** PAULI, W. *et al.*, Eds. (1955)

LANDÉ, A. (1959) From dualism to unity in quantum mechanics. **10,** 16–24

LANDÉ, A. (1961) **R** YOURGRAU, W. and MANDELSTAM, S. (1960)

LANDÉ, A. (1965) Why do quantum theorists ignore the quantum theory? **15,** 307–13

LANDSBERG, P. T. (1951) **R** von FREYTAG L. B. (1948)

LAUDAN, L. (1966) **R** COLODNY, R. G., Ed. (1965)

LAUDAN, L. (1967) **R** BUNGE, M., Ed. (1964)

LAUDAN, L. (1968) **R** SCHLEGEL, R. (1967)

LAUDAN, L. (1969) **R** KOYRÉ, A. (1968)

LAZEROWITZ, M. (1958) **R** CRAWSHAY-WILLIAMS, R. (1957)

LEACH, E. (1969) **R** COHEN, P. S. (1968)

LEACH, J. (1968) Explanation and value neutrality. **19,** 93–108

LEBLANC, H. (1960) **N** POPPER, K. R. (1954). **D** POPPER, K. R. (1960) **10,** 315–18

LEHMAN, H. S. (1965) **N** CANFIELD, J. (1964)

LEJEWSKI, C. (1954) Logic and existence. **5,** 104–19. **D** PRIOR, A. N. (1955) **6,** 64–5

LEJEWSKI, C. (1955) **R** REICHENBACH, H. (1954)

LEJEWSKI, C. (1956) **R** APOSTLE, H. G. (1952)

LEJEWSKI, C. (1957) **R** WEDBERG, A. (1955)

LEJEWSKI, C. (1958) **R** MEYER, H. (1956)

LEJEWSKI, C. (1958) **R** CARNAP, R. (1956)

LEJEWSKI, C. (1959) **R** GOODMAN, N. (1954)

LEJEWSKI, C. (1963) **R** WHITE, M. (1956)

van LENNEP, D. J. (1961) **R** POLANYI, M. (1959)

LEPLIN, J. (1969) Meaning variance and the comparability of theories. **20,** 69–75

LEVI, I. (1963) **N** POPPER, K. R. (1955)

LEVI, I. (1967) Probability kinematics. **18,** 197–209

LEVI, I. (1968) **R** HINTIKKA, J. and SUPPES, P., Eds. (1966)

Levi, I. (1968) **R** Salmon, W. C. (1966)

Levi, I. (1969) **N** Kyburg Jr., H. E. and Harper, W. L. (1968)

Levinson, H. B. (1965) **N** Keswani, G. H. (1960); (1961) **N** Born, M. (1961)

Lieb, I. C. (1967) **R** Moore, E. C. and Robin, R. S., Eds. (1964)

Lloyd, G. E. R. (1966) **R** Allen, R. E., Ed. (1965)

Lloyd, G. E. R. (1966) **R** Crombie, I. M. (1964)

Lloyd, G. E. R. (1967) Popper versus Kirk: a controversy in the interpretation of Greek science. **18,** 21–38

Long, W. (1953) **N** Watson, G. (1951)

Lonsdale, K. (1951) **R** Leake, C. D. and Romanell, P. (1950)

Lonsdale, K. (1952) **R** Schrödinger, E. (1951)

Louch, A. R. (1962) Science and psychology. **12,** 314–27

Lucas, J. R. (1968) **N** Good, I. J. (1967)

Lucas, J. R. (1969) *Euclides ab omni naevo vindicatus.* **20,** 1–11

Lucas, J. R. (1969) **R** Ayers, M. R. (1968)

Lyon, A. (1967) Causality. **18,** 1–20

MacDonald, D. K. C. (1960) **N** Caldin, E. F. (1959)

MacIntyre, A. (1968) **R** Rudner, R. S. (1966)

MacIntyre, A. (1969) **R** Galtung, J. (1967)

MacIntyre, A. (1969) **R** Brodbeck, M., Ed. (1968)

Mackay, D. M. (1951) Mindlike behaviour in artefacts. **2,** 105–21 **D** Pirenne, M. H. (1952)

MacKay, D. M. (1953) **N** Mays, W. (1952)

MacKay, D. M. (1953) **R** Milne, E. A. (1952)

MacKay, D. M. (1960) **R** Coulson, C. A. (1958); Feibleman, J. K. (1958)

MacKay, D. M. (1960) **R** Farrer, A. (1958)

MacKay, D. M. (1962) The use of behavioural language to refer to mechanical processes. **13,** 89–103. **D** Fozzy, P. J. (1963) **14,** 154–7; MacKay, D. M. (1963) **14,** 157–9

MacKay, D. M. (1965) **R** Culbertson, J. T. (1963)

MacKay, D. M. (1965) **R** Bar-Hillel, Y. (1964)

MacKay, D. M. (1967) **R** Barbour, I. G. (1966)

Mackie, J. L. (1963) The paradox of confirmation. **13,** 265–77. **D** Gibson, L. (1969) **20,** 75–80

Mackie, J. L. (1965) **R** Scheffler, I. (1964)

Mackie, J. L. (1966) **N** Miller, D. (1966)

Mackie, J. L. (1968) **R** Körner, S. (1966)

Mackie, J. L. (1968) **R** Levi, I. (1967)

Mackie, J. L. (1969) The relevance criterion of confirmation. **20,** 27–40

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McCrea, W. H. (1950) **R** Whitrow, G. J. (1949)

McCrea, W. H. (1951) **R** Maziarz, E. A. (1950)

McCrea, W. H. (1951) **R** Weyl, H. (1949)

McCrea, W. H. (1952) **R** Hilbert, D. and Ackermann, W. (1950)

McCrea, W. H. (1952) **R** Landau, E. (1951)

McCrea, W. H. (1953) **R** Speiser, A. (1952)

McCrea, W. H. (1957) On the objective of Einstein's work. **8,** 18–29

McCulloch, W. S. (1954) Through the den of the metaphysician. **5,** 18–31

McKinsey, J. C. C. and Suppes, P. (1955). On the notion of invariance in classical mechanics. **5,** 290–302

Madden, E. H. (1957) A logical analysis of 'psychological isomorphism'. **8,** 177–91

Magyar, G. (1965) On the dual nature of light. **16,** 44–9

Mandelbaum, M. (1957) Societal laws. **8,** 211–24

Marshall, N. (1960) ESP and memory: a physical theory. **10,** 265–86. **D** Dobbs, H. A. C. (1961) **12,** 65–8; Marshall, N. (1961) **12,** 68–70

Marshall, N. (1961) **N** Dobbs, H. A. C. (1961)

Martin, A. (1957) How economic theory may mislead. **8,** 225–36

Martin, A. (1962) Epistemology as a liberal polemic. **R** von Mises, L. (1960)

Martin, A. (1964) Empirical and *a priori* in economics. **15,** 123–36

Martin, J. R. (1969) Another look at the doctrine of *verstehen*. **20,** 53–67

Maruyama, M. (1959) Communicable and incommunicable realities. **10,** 50–4

Maruyama, M. (1961) Communicational epistemology. **11,** 319–27; **12,** 52–62; 117–31

Mascall, E. L. (1954) **R** Ramsey, I. T. (1952)

Maxwell, A. N. (1969) **R** Brody, B. A. and Capaldi, N. C., Eds. (1968)

Maxwell, A. N. (1969) **R** Kockelmans, J., Ed. (1968)

Maxwell, N. (1966) Physics and common sense. **16,** 295–311

Maxwell, N. (1968) Can there be necessary connections between successive events? **19,** 1–25

Maxwell, N. (1969) **R** Lakatos, I. and Musgrave, A., Eds. (1968)

Mays, W. (1951) **N** Wisdom, J. O. (1951)

Mays, W. (1952) **N** Pirenne, M. H. (1952). **D** MacKay, D. M. (1953) **3,** 352–3

Mays, W. (1956) **R** Northrop, F. S. C. and Gross, M. W. (1953)

Mays, W. (1956) **R** Césari, P. (1955)

Mays, W. (1957) **R** Black, M. (1954)

Mays, W. (1959) **R** Estes, W. K. and Suppes, P. (1957)

Mays, W. (1960) History and philosophy of science in British Commonwealth Universities. **11,** 192–211

Mays, W. (1964) **R** Turbayne, C. M. (1962)

Mays, W. (1964) **R** Michotte, A. (1963)

Medawar, P. B. (1954) **R** Bean, R. B. and W. B., Eds. (1950)

Meek, R. L. (1964) Value-judgements in economics. **15,** 89–99

Mellor, D. H. (1965) Connectivity, chance, and ignorance. **16,** 209–25. **D** Mellor, D. H. (1967) **18,** 235–8

Mellor, D. H. (1967) **R** Ellis, B. (1966)

Mellor, D. H. (1967) **N** Mellor, D. H. (1965)

Mellor, D. H. (1969) **R** Swinburne, R. (1968)

Meltzer, B. and Good, I. J. (1965) Two forms of the prediction paradox. **16,** 50–1

Menger, K. (1954) On variables in mathematics and in natural science. **5,** 134–42

Michalos, A. C. (1966) Estimated utility and corroboration. **16,** 327–31

Miles, T. R. (1957) On the difference between men and machines. **7,** 277–92

Miller, A. R. (1951) **R** Hooper, A. (1949)

Miller, D. (1966) A paradox of information. **17,** 59–61. **D** Popper, K. R. (1966) **17,** 61–9; MacKie, D. (1966) **17,** 144–7; Miller, D. (1966) **17,** 147–9; Bub, J. and Radner, M. (1967) **19,** 63–7; Miller, D. (1968) **19,** 145–52; Rozeboom, W. W. (1969) **19,** 345–53

Miller, D. (1966) **N** Miller, D. (1966)
Miller, D. (1968) **N** Miller, D. (1966)
Milligan, D. E. (1967) **R** Cohen, R. S. and Wartofsky, M. W., Eds. (1965)
Mintz, S. I. (1966) **R** Watkins, J. W. N. (1965)
de Monchaux, C. (1959) **R** Szasz, T. S. (1957)
Money-Kyrle, R. E. (1956) The world of the unconscious and the world of commonsense. **7,** 86–96
Morrison, P. G. (1960) On partial identity of cause and effect. **11,** 42–9
Mueller, I. (1969) Euclid's *Elements* and the axiomatic method. **20,** 289–309
Mundle, C. W. K. (1954) **N** Dobbs, H. A. C. (1951)
Mundle, C. W. K. (1956) **R** Soal, S. G. and Bateman, F. (1954)
Mundle, C. W. K. (1957) **R** Jung, C. G. and Pauli, W. (1955)
Munitz, M. K. (1954) Creation and the 'new' cosmology. **5,** 32–46
Munitz, M. K. (1962) The logic of cosmology. **13,** 34–50
Musgrave, A. E. (1969) **R** Ziman, J. M. (1968)
Nagel, E. (1959) **R** Hutten, E. H. (1956)
Narlikar, J. V. (1965) The direction of time. **15,** 281–5
Narlikar, J. V. (1968) **R** Prokhovnik, S. J. (1967)
Needham, R. M. (1966) **R** Bagrit, L. (1965)
Nerlich, G. C. and Suchting, W. A. (1967) Popper on law and natural necessity. **18,** 233–5. **D** Popper, K. R. (1967) **18,** 316–21; Suchting, W. A. (1969) **20,** 349–52
Newton-Smith, W. (1969) **R** Gale, R. M. (1968)
North, J. D. (1966) **R** Morris, C. (1964)
North, J. D. (1967) **R** Fraser, J. T., Ed. (1966)
O'Brien, D. (1969) **R** Sinnige, T. G. (1968)
O'Connor, D. J. (1954) **R** von Wright, G. H. (1951)
O'Connor, D. J. (1955) **R** Hesse, M. B. (1954)
O'Connor, D. J. (1956) **R** Pap, A. (1955)
O'Connor, D. J. (1956) **R** Février, P. (1955)
O'Connor, D. J. (1957) Determinism and predictability. **7,** 310–15
O'Connor, D. J. (1963) **R** Chisholm, R. M., Ed. (1960)
Ogden, C. K. (1951) **R** Brown, G. B. (1950)
Öpik, E. J. (1954) The age of the universe. **5,** 203–14
Oppenheim, P. and Rescher, N. (1955) Logical analysis of Gestalt concepts. **6,** 89–106
Padel, J. (1964) **R** Feigl, H. and Scriven, M., Eds. (1956)
Paneth, E. (1963) **N** Bradley, J. (1962)
Paneth, F. A. (1962) The epistemological status of the chemical concept of element. **13,** 1–14; 144–60. **D** Bradley, J. (1962) **13,** 316–17; Paneth, F. A. (1962) **13,** 317–18
Pantin, C. F. A. (1960) Chairman's comments on the contributions by Woodger and Hutten. **11,** 109–12
Pap, A. (1952) The concept of absolute emergence. **2,** 302–11
Peters, R. S. (1956) Freud's theory. **7,** 4–12
Peters, R. S. and Tajfel, H. (1957) Hobbes and Hull—metaphysicians of behaviour. **8,** 30–44
Peters, R. S. (1967) **R** Barry, B. (1965)

Pikler, A. G. (1954) Utility theories in field physics and mathematical economics. **5**, 47–58; 303–18

Pirenne, M. H. (1950) Descartes and the body-mind problem in physiology. **1**, 43–59

Pirenne, M. H. (1952) **N** MacKay, D. M. (1951). **D** Mays, W. (1952) **3**, 191–3

Pirenne, M. H. (1952) The scientific basis of Leonardo da Vinci's theory of perspective, **3**, 169–85

Pirenne, M. H. (1953) Physiological mechanisms in the perception of distance by sight and Berkeley's theory of vision. **4**, 13–21

Pirenne, M. H. (1954) **R** Eccles, J. C. (1953)

Pirie, N. W. (1952) Concepts out of context: the Pied Piper of science. **2**, 269–280. **D** Carter, G. S. (1952) **3**, 86–7

Pirie, N. W. (1956) **R** Oparin, A. I. (1953)

Plaut, H. C. (1960) Conditions, cause, free will, and the direction of time. **11**, 212–21

Plaut, H. C. (1962) Empiricism, solipsism, and realism. **13**, 216–28

Plaut, H. C. (1963) **N** Schlick, M. (1961)

Polanyi, M. (1952) The stability of beliefs. **3**, 217–32

Polanyi, M. (1952) **N** Wisdom, J. O. (1951)

Polanyi, M. (1957) Problem solving. **8**, 89–103

Polanyi, M. (1967) Science and reality. **18**, 177–96

Pollock, S. (1958) **N** Gallie, W. B. (1957)

Poltawski, A. (1969) **R** Bergmann, G. (1967)

Poole, B. and Frankfurt, H. G. (1966) **N** Canfield, J. (1964)

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Popper, K. R. (1955) Two autonomous axiom systems for the calculus of probabilities. **6**, 51–7

Popper, K. R. (1955) **N** Bar-Hillel, Y. (1955) **D** Carnap, R. (1956) **7**, 243–4; Popper, K. R. (1956) **7**, 244–5; Bar-Hillel, Y. (1956) **7**, 245–8; Popper, K. R. (1956) **7**, 249–56; Levi, I. (1962) **13**, 307–13

Popper, K. R. (1956) **N** Carnap, R. (1956) **D** Achinstein, P. (1962) **13**, 167–71

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Popper, K. R. (1958) On Mr Roy Harrod's new argument for induction. **9**, 221–4

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Popper, K. R. (1959) The propensity interpretation of probability. **10**, 25–42

Popper, K. R. (1959) **N** Grünbaum (1959). **D** Keswani, G. H. (1960) **11**, 50–5

Popper, K. R. (1960) **N** Leblanc, H. (1960)

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Popper, K. R. (1966) **N** Popper, K. R. (1966)

Popper, K. R. (1968) **N** Nerlich, G. C. and Suchting, W. A. (1967). **D** Suchting (1969) **20,** 349–52

Post, H. R. (1960) Retrospective miracles, or betting after the race. **R** Rhine, J. B. and Pratt, J. G. (1957)

Post, H. R. (1960) Simplicity in scientific theories. **11,** 32–41

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Post, H. R. (1962) **R** Schrödinger, E. (1961)

Pottage, J. (1969) **R** Klein, J. (1968)

Potter, O. (1953) **N** On the function of chemical formulae: a nineteenth-century discussion. **3,** 359–63

Presley, C. F. (1961) Arguments about meaninglessness. **12,** 225–34

Price, H. H. (1956) **R** Kapp, R. O. (1955)

Price, H. H. (1959) **R** Chisholm, R. M. (1957)

Price, H. H. (1960) Sir Russell Brain on modes of apprehension. **R** Brain, R. (1959)

Primack, M. (1968) **R** Mandelbaum, M. (1967)

Pringle, J. W. S. (1953) **R** Hyndman, O. R. (1952)

Prior, A. N. (1955) **N** Lejewski, C. (1954)

Prior, A. N. (1969) **R** von Wright, G. H. (1969)

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Prokhovnik, S. J. (1963) The case for an aether. **14,** 195–207

Prokhovnik, S. J. (1968) **N** Popper, K. R. (1966)

Pryce, M. H. L. (1960) **R** Born, M. (1956)

Przelecki, M. (1962) **R** Bergmann, G. (1960)

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Puccetti, R. (1967) On thinking machines and feeling machines. **18,** 39–51

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Quine, W. V. (1957) The scope and language of science. **8,** 1–17

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Radner, M. and Bub, J. (1968) **N** Miller, D. (1966); Popper, K. R. (1966)

Rasmussen, E. T. (1953) Berkeley and modern psychology. **4,** 2–12

Rattansi, P. M. (1967) **R** Jones, R. F. (1965)

Rattansi, P. M. (1968) **R** Debus, A. G. (1968)

Ravetz, J. R. (1960) **R** Moritz, R. E. (1958)

Ravetz, J. R. (1960) **R** Bridgman, P. W. (1959)

Ravetz, J. R. (1960) **R** Blin-Stoyle, R. J. *et al.* (1959)

Ravetz, J. R. (1961) **R** Gillespie, C. C. (1960); Buchdahl, G. *et al.* (1960)

Ravetz, J. R. (1962) **R** Land, F. W. (1960); Hogben, L. (1960)

Ravetz, J. R. (1965) **R** Manuel, F. E. (1963)

Rawlins, I. (1950) **R** Kruse, V. (1949)

Rawlins, I. (1951) **R** Urban, W. M. (1949)

Rawlins, I. (1951) **R** Ardley, G. (1950)

Rawlins, I. (1951) **R** Wittkower, R. (1949)

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Rescher, N. (1958) On prediction and explanation. **8,** 281–90

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Richens, R. H. (1951) **R** Lunn, A. (1950)

Richens, R. H. (1952) **R** Vavilov, N. I. (1951)

Ritchie, A. M. (1962) **R** Descartes, R. (1960)

Robinson, R. (1965) **R** Koestler, A. (1964)

Robinson, R. E. (1968) **R** Jeffrey, R. C. (1965)

Rosenfeld, L. (1958) **R** de Broglie, L. (1956)

Roshwald, M. (1955) Value-judgements in the social sciences. **6,** 186–208. **D** Grünbaum, A. (1956) **7,** 236–40

Roshwald, M. (1956) **N** Grünbaum, A. (1956)

Rozeboom, W. W. (1969) **N** Miller, D. (1966)

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Russell, L. J. (1954) Note on a passage in Berkeley's *De Motu.* **5,** 152

Russell, L. J. (1955) **R** Ryle, G. (1954). **D** Brown, G. B. (1955) **6,** 251–2

Russell, L. J. (1955) **R** Hermathena (1953)

Russell, L. J. (1955) **R** Nagel, E. (1954)

Russell, L. J. (1956) **R** Saw, R. L. (1954)

Russell, L. J. (1957) **R** International Federation of Modern Literature and Science (1955)

Russell, L. J. (1960) **R** Fleckenstein, J. O. (1958)

Russell, R. W. (1951) The comparative method in the study of behaviour. **2,** 251–5

Russell, R. W. (1952) **R** Beveridge, W. I. B. (1950)

Sabra, A. I. (1954) A note on a suggested modification of Newton's corpuscular theory of light to reconcile it with Foucault's experiment of 1850. **5,** 149–51.

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SPILSBURY, R. J. (1953) **R** SINCLAIR, A. (1951)
SPILSBURY, R. J. (1955) **R** ROUSSEL, J., Ed. (1954)
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SPILSBURY, R. J. (1960) **R** HARRIS, E. E. (1959)
SPROTT, W. J. H. (1953) **R** HAYEK, F. A. (1952)
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SYNGE, J. L. (1962) **R** WHITROW, G. J. (1961)

Synge, J. L. (1962) **R** Whyte, L. L. (1961)
Szasz, I. S. (1956) **N** Walker, N. (1956). **D** Walker, N. (1956) **7**, 235–6
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Taylor, F. S. (1952) **R** Read, J. (1947)
Teichmann, M. (1962) Psychoanalysis and scientific method. **R** Hook, S., Ed. (1959)
Teller, P. (1969) Goodman's theory of projection. **20**, 219–38
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Walker, N. (1956) Freud and homeostasis. **7**, 61–72
Walker, R. C. S. (1969) **R** Danto, A. C. (1968)
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Watkins, J. W. N. (1955) **R** Cohen, M. R. (1953)

Watkins, J. W. N. (1955) **R** Shackle, G. L. S. (1952)
Watkins, J. W. N. (1955) **R** Acton, H. B. (1955)
Watkins, J. W. N. (1957) **R** Shackle, G. L. S. (1955)
Watkins, J. W. N. (1957) Historical explanation in social sciences. **8,** 104–17
Watkins, J. W. N. (1959) **N** Goldstein, L. J. (1958). **D** Goldstein, L. J. (1959) **10,** 240–1; Watkins, J. W. N. (1959) **10,** 242–4
Watkins, J. W. N. (1959) **R** Argyle, M. (1957)
Watkins, J. W. N. (1959) **N** Goldstein, L. J. (1959)
Watkins, J. W. N. (1959) **R** Radcliffe-Brown, A. R. (1957)
Watkins, J. W. N. (1960) When are statements empirical? **10,** 287–308
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Watkins, J. W. N. (1965) Mandelbaum's critical realism. **R** Mandelbaum, M. (1964)
Watling, J. (1964) **R** Toulmin, S. (1961)
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Whitrow, G. J. (1957) **R** Mascall, E. L. (1956)
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Whitrow, G. J. (1958) **R** Menger, K. (1955)
Whitrow, G. J. (1958) **R** Dugas, R. (1954)
Whitrow, G. J. (1958) **R** Douglas, A. V. (1956)
Whitrow, G. J. (1959) **R** Munitz, M. K. (1957)
Whitrow, G. J. (1961) **R** Cohen, I. B. and Schofield, R. E., Eds. (1958)
Whitrow, G. J. (1961) **R** Newton, I. (1959 and 1960)
Whitrow, G. J. (1962) **R** Whyte, L. L. (1961)
Whitrow, G. J. (1963) **R** Newton, I. (1962)

Whitrow, G. J. (1963) **R** Turnbull, H. W., Ed. (1961)
Whitrow, G. J. (1968) **R** Scott, J. F., Ed. (1967)
Whitrow, G. J. (1969) **R** Koslow, A., Ed. (1967)
Whittaker, E. T. (1951) **R** Beth, E. W. (1950)
Whittaker, E. (1953) **N** Schrödinger, (1952)
Whyte, L. L. (1951) **R** Brussels Symposium (1948)
Whyte, L. L. (1951) **R** Langer, S. K. (1942)
Whyte, L. L. (1951) Fundamental physical theory. **1**, 303–27. **D** Whyte, L. L. (1952) **3**, 256–8
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Whyte, L. L. (1952) **R** Thiel, M., Ed. (1951)
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Whyte, L. L. (1953) **N** Schrödinger, E. (1952)
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Whyte, L. L. (1960) **R** Cope, J. I. and Jones, H. W., Eds. (1959)
Whyte, L. L. (1963) Some thoughts on certainty in physical science. **14**, 32–8
Whyte, L. L. (1967) **R** Burgers, J. M. (1965)
Whyte, L. L. (1969) **R** Brunner, C. (1968)
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Wilkie, J. S. (1953) **R** Grassi, E. and von Uexküll, T. (1950)
Wilkie, J. S. (1958) **R** Butler, J. A. V. (1957)
Wilkins, B. T. (1964) Collingwood reconsidered. **R** Donagan, A. (1962)
Wilkins, B. T. (1965) What is history? **R** Gallie, W. B. (1964)
Williams, L. P. (1960) **R** Sarton, G. (1959)
Williams, L. P. (1960) **R** Clagett, M. (1959)
Williams, L. P. (1966) **R** Hesse, M. B. (1961)
Williams, L. P. (1967) **N** Kuhn, T. S. (1967)
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Wilson, F. (1968) Definition and discovery. **18**, 283–303; **19**, 43–56
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Wilson, P. R. (1964) On the confirmation paradox. **15,** 196–9
Winnie, J. A. (1967) The implicit definition of theoretical terms. **18,** 223–9
Wisdom, J. O. (1950) **R** Caldin, E. F. (1949)
Wisdom, J. O. (1951) The hypothesis of cybernetics. **2,** 1–24. **D** Walshe, F. M. R. (1951) **2,** 161–3; Adkins, B. M. (1951) **2,** 248; Mays, W. (1951) **2,** 249–50; Polanyi, M. (1952) **2,** 312–15
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Wisdom, J. O. (1953) **R** Bailey, S. (1842); Abbott, T. K. (1864)
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Wisdom, J. O. (1954) **R** Basson, A. H. and O'Connor, D. J. (1953)
Wisdom, J. O. (1954) **R** Pap, A. (1953)
Wisdom, J. O. (1954) **R** Smith, N. K. (1952)
Wisdom, J. O. (1954) **R** Peters, R. S., Ed. (1953)
Wisdom, J. O. (1954) **R** Jessop, T. E. (1953)
Wisdom, J. O. (1954) **R** Luce, A. A. and Jessop, T. E., Ed. (1953)
Wisdom, J. O. (1954) **R** Woodger, J. H. (1952)
Wisdom, J. O. (1955) **R** Anscombe, G. E. M. and Geach, P. T., Eds. (1954)
Wisdom, J. O. (1955) **R** Abramson, H. A. (1951–4)
Wisdom, J. O. (1956) Psycho-analytic technology. **7,** 13–28
Wisdom, J. O. (1956) **R** Klein, M. *et al.*, Eds. (1955)
Wisdom, J. O. (1956) **R** Pumpian-Mindlin, E., Ed. (1952)
Wisdom, J. O. (1956) **R** Feigl, H. and Brodbeck, M., Eds. (1953)
Wisdom, J. O. (1956) **R** Scott, J. W., Ed. (1954)
Wisdom, J. O. (1957) **R** Coleman, J. A. (1954)
Wisdom, J. O. (1957) **R** Neurath, O. *et al.*, Eds. (1956)
Wisdom, J. O. (1958) **R** Sartre, J-P. (1953)
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Wisdom, J. O. (1958) **R** Themerson, S. (1956)
Wisdom, J. O. (1958) **R** Mannheim, H. and Wilkins, L. T. (1955)
Wisdom, J. O. (1958) **N** Das, R. (1958)
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Withers, R. F. J. (1952) **R** Reichenbach, H. (1951)
Withers, R. F. J. (1953) **R** von Bertalanffy, L. (1952)
Withers, R. F. J. (1955) **R** Arber, A. (1954)
Withers, R. F. J. (1956) **R** Colby, K. M. (1955)
Withers, R. F. J. (1957) **R** Gregg, J. R. (1954)
Withers, R. F. J. (1957) **R** Cardwell, D. S. L. (1957)

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Withers, R. F. J. (1962) **R** Goodfield, G. J. (1960)
Wollaston, A. (1959) **R** Joachim, H. H. (1957)
Woodger, J. H. (1951) **R** Butterfield, H. (1950)
Woodger, J. H. (1951) **R** Frank, P. (1949
Woodger, J. H. (1951) Scpience without roperties. **2,** 193–216
Woodger, J. H. (1952) **R** Becker, O. (1951)
Woodger, J. H. (1952) **R** Flew, A., Ed. (1951)
Woodger, J. H. (1952) From biology to mathematics. **3,** 1–21
Woodger, J. H. (1952) **R** Curry, H. B. (1952)
Woodger, J. H. (1953) What do we mean by 'inborn'? **3,** 319–26. **D** Haldane, J. B. S. (1955) **6,** 245–8
Woodger, J. H. (1953) **R** Lukasiewicz, J. (1951)
Woodger, J. H. (1956) **N** Haldane, J. B. S. (1955)
Woodger, J. H. (1958) **R** Menger, K. (1957)
Woodger, J. H. (1958) **R** Carnap, R. (1954)
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Yolton, J. W. (1959) Explanation. **10,** 194–208
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Powers

by R. HARRÉ

TWO PARADIGMS OF ACTION

I propose to show that the concept of 'power' can play a central role in a metaphysical theory appropriate to a realist philosophy of science. To begin I want to describe and contrast two main paradigms of action. *Paradigm* 1: *Descartes' Paradigm.* A stationary ball resting on a smooth table is struck by a moving ball. It begins to move and continues moving. The ball which struck the stationary ball continues to move but less quickly. We are strongly tempted to think of this happening as a transaction in which the totality of the effect is due to the originally moving ball, and that the stationary ball contributes nothing to the final situation. The second ball which originally was stationary has no intrinsic power of motion, though it can move. Looking deeper and further into this paradigm, we ask about the first ball, that which was originally moving. Did it possess its motion intrinsically, did it move of itself? By reapplication of the paradigm it can be inferred that the first ball must have received an impetus to motion from something other than itself and that all its motion must have been acquired from sources external to itself. By continual reapplication of the paradigm, it can be concluded that there is no ball, not even a First or Primal Ball which has an intrinsic power of motion. This paradigm while exercising, I believe, a powerful influence over our thought, is not a straight metaphysician's crib from the science of mechanics. In that science, the final state of a system of bodies is a product of two factors, an extrinsic factor which is the various states of motion of the balls before they collide, and an intrinsic factor, which is the inertia or mass of the balls. Inertia is, as it were, a negative power, the power to resist indefinite increments of motion. But this shadowy negative sort of power has little metaphysical excitement about it, and the paradigm is, for those attracted by it, still much as Descartes expressed it. The problem of the origin of motion can be solved either by conceiving of the hand of God setting every piece of matter in motion in the beginning

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and so arranging the laws of nature that the total quantity of that commodity is conserved; or by conceiving of the Universe as infinitely old, and so refusing the application of any questions of origin. The application of the Cartesian paradigm requires us to suppose that in all action the effect is completely produced by the impressed or stimulating cause.

The exigencies of fact require the modification of the paradigm in practice. The buffetings to which things (including persons and animals, and magnetic tapes) are subjected, changes them intrinsically, as well as stimulating them to respond. These changes in the natures and constitutions of things subsequently have an effect on the responses which the things can make to impressed stimuli. Continually bashing away at Hume's billiard ball will eventually knock bits off it; so that if it acquires, for example, the same increment of momentum every time something collides with it, it will move off more quickly each time, leaving a few chips of ivory behind.

Such modifications are in the direction, but not very far in the direction of another paradigm.

Paradigm 2: Van Helmont's Paradigm. On a fine sunny afternoon, of only moderate heat, and with no breeze to speak of, a man dozes in deck chair in a garden. There are no flies, nor mosquitoes, nor wasps, nor shouts from neighbours' children. Suddenly the man jumps up, walks smartly to the shed, takes out the lawn mower and begins to mow the lawn. Nothing extrinsic to him had changed. The subsequent changes, for example the smelly racket of the motor mower, are entirely the products of the action of the man, and the ultimate causes of which are to be found among states intrinsic to him.

But most cases of human action are not quite like that. This paradigm too gets modified. The man in the garden had the power to act and the capacity to be stimulated to act. He might have begun to mow because he was asked to do so. But a stick of dynamite or a loaded gun do not fit this paradigm, since their powers, though in some way conditional on the existence of certain intrinsic states, are exercised only in the appropriate circumstances, and under suitable stimuli. To say that nitroglycerine is a stable explosive is not to denigrate its power to blast but to intimate that it will manifest it only when appropriately stimulated. To say that ammonium tri-iodide is an unstable explosive is to intimate that whatever care you may take to isolate it from interference from without it has been known to go off, *by itself*, as we say. We begin to assimilate it to the paradigm of the sleeping gardener.

If we consider the totality of our knowledge of things, materials, and persons one would be much inclined to say that neither paradigm of action is exemplified without qualification, but that a wide range of different proportions of responsibility is assigned on the one hand to the constitution or nature or powers of things, materials, and persons, and on the other to the stimuli to which they respond, i.e. to intrinsic states versus extrinsic circumstances.

On the official metaphysics of positivism, what could we possibly make of spontaneity, of the permanent possession of powers by things and substances, of a kind of causality where the cause and the conditions in which it acts are very much subordinate in their responsibility for whatever effects take place, to the intrinsic natures and constitutions of things?

Clearly the Cartesian paradigm is enshrined in the metaphysics of positivism. If things are nothing but the collocations of their manifested qualities, whether this is given a phenomenalist ring or not, and if causality is but regularity of sequence of like pairs of events, and events are what happens *to* things, there can be no place for active powers.

There has seemed to be something fishy and soft, occult and mysterious about the second paradigm, and something tough, scientific and empirical about the first paradigm. But what are things, materials and persons really like? Are they like sitting ducks and stationary billiard balls, or are they like loaded guns and sticks of dynamite? Why has the second paradigm seemed fishy when it seems to be so natural and so clearly forced upon us by the way things are? Part of the anwer lies in a mistaken epistemology which confines the data and thus the content of science to simple truths about sensory qualities manifesting themselves to an observer in particular circumstances, so that all that science can really be about is the obvious and the overt, and all laws but the statistics of the obvious and the overt. It is also partly due to a mistaken metaphysics, in which 'power' is seen as a concept surviving from magic, an occult quality appealing only to those of too tender a mind to face the stern truths of empiricism. I shall show in this paper that not only are powers indispensable in the epistemology of science, but are the very heart and key to the best metaphysics for science. In so doing, I shall show that the concept of power is neither magical nor occult but is as empirical a concept as we could well ask for, yet richer in capacity than those concepts it succeeds.

POWERS AND DISPOSITIONS

In widening the sources of paradigms for action from the Cartesian patient to the spectrum between patient and agent and in treating this as deriving from the difference between intrinsic conditions and extrinsic circumstances, I have assumed an intuitive understanding of the concept of power. This must be made good by an analysis or explanation of what is being said of a thing or material when a power or powers are ascribed to it. And here we groom an old and familiar concept for a technical role. What sort of description of the thing or of the material is now being given? It is clearly *not* a simple qualitative description of the thing or material. Simple, qualitative descriptions such as 'This one is red' or 'That stuff is cold' are intended to ascribe just that sensory quality that is then and there manifested by the thing or substance. But to say that that thing is brittle, that stuff is explosive, and the like are not that sort of ascription.

The analysis of the ascription of such properties has been the occasion for the introduction of the idea of a disposition. Similar notions such as propensity, trend, tendency, and liability have been treated in an essentially similar way.

Under the influence of Ryle (1949, Ch. 5(2)) and others a particular way of handling all these ascriptions has become widespread. We are recommended to treat all of these ascriptions not as genuine categorical assertions of the presence of a quality, but to analyse them all as hypotheticals or conditional statements, or conjunctions of such statements. Any fine differences in import are supposed to be expressed by the use of the indicative and subjunctive moods. The meaning of 'It is brittle' is to be 'if maltreated, it will break', together with a trail of subjunctive conditionals of varying degrees of specificity trailing after it, whose role is to capture the force of ascriptions of brittleness to those things which did not, or have not yet broken. In a similar spirit 'It is poisonous' is to mean 'if taken, it will kill or make ill', and 'It can crush a car' is to mean 'if it presses a car, the car will be reduced to the size of a suitcase'. 'He is intelligent' is to mean 'if he is presented with a problem, he will quickly produce the solution'. Since the kind of problem is not specified, such ascriptions are analysed as generic rather than a specific hypotheticals, that is to say an unspecified range of specific dispositions are implied by the generic hypothetical.

The problem of what the assertion of a power or disposition to a thing means when it is not exercising it is not really solved for this analysis. To say that to assert that a particular piece of glass *is* brittle is to make a prediction about how it *would* behave if certain conditions were fulfilled is not enough, since it leaves the intractable problem of the truth-conditions of the subjunctive conditional conjunct in the analysis unresolved. At best one might agree that the ascription of a disposition may perhaps be appropriately treated this way, but this technique cannot work with the concept of a power or liability. Things and materials *have* powers even when they are not exercising them, and that is a current fact about them, a way in which they are currently differentiated from other things and materials which lack and now lack these powers. Indeed, ***the reason why we believe that a certain disposition can be asserted truly of a thing or material is that we think or indeed know that it currently has such and such powers.***[1] One of our reasons, and sometimes it is the only reason, for believing that if certain conditions are fulfilled then a material or an individual will behave in a certain way, is that the thing or material now has the power to behave in that way, should the conditions obtain. The difference between something which has the power to behave in a certain way and something which does not have that power is not a difference between what they *will do*, since it is contingently the case that their powers are, in fact, ever elicited, but it is a difference in what they themselves now are. It is a difference in intrinsic nature.

THE ANALYSIS OF POWER ASCRIPTIONS

The proper analysis of the ascription of a power to a thing or material (and with some qualifications, also to a person) is this:

> X has the power to A = if X is subject to stimuli or conditions of an appropriate kind, then X will do A, ***in virtue of its intrinsic nature.***

The last clause is vital, and marks the difference between the ascription of powers and any other kind of description. In ascribing powers to people 'can' must be substituted for 'will' in the apodasis of the hypothetical component. Whether he will or no is up to him.

From this follow two vital points, and these really explain why we must have the concept of power for making sense of science.

(i) To ascribe a power to a thing or material is to say something specific about what it *will* or *can do*, but to say something unspecific about what it *is*. That is, to ascribe a power to a thing asserts only that it can do what it does in virtue of its nature, whatever that is. It leaves open the question of the exact specification of the nature or constitution in virtue of which it has the power. Perhaps that can be discovered by empirical investigation later.

(ii) But to ascribe a power is to say that what the thing or material does or can do is to be understood as brought about not just by the stimuli to which it may be subject or the conditions which it finds itself in, but in some measure by the nature or constitution that the thing or material has. That is why the ascription of a power is a schematic explanation.

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[1] A similar point, starting from a different aspect of the problems of dispositional analyses, is to be found in Levi and Morgenbesser (1964) particularly Part III.

It might be argued that the use of the structure 'If *a* is *C* then *a* will be *D* in virtue of being *N*', for expressing the logical form of a power attribution to a thing or material, with its puzzling 'in virtue of' clause, is unnecessary, on the grounds that the maintenance of the nature of the thing or material could be treated as one of the conditions which has to obtain for it to behave in the ascribed manner. But there are distinctions which this simple conditional form obscures.

There is the distinction between *extrinsic circumstances*, changes in which are not properly to be considered to be changes in the thing or material *itself*, and *intrinsic conditions* which are a proper part or feature of the thing itself. There are certain subtle considerations, particularly important in handling the conceptual structure of human powers, by which this distinction is differentiated from that between *internal* and *external* conditions of the manifestation of a power. Internal conditions lie within the spatial envelope of the thing, and external conditions outside it. It is not always true that the whole of the conditions pertinent to the nature of a thing or material are wholly inside its physical envelope. For example, the state of the magnetic field external to a thing may be an intrinsic condition considered with respect to its behaviour, since the nature of the body in virtue of which it has certain dispositions may derive in part from the state of the whole field within which it lies. An intrinsic condition is not necessarily an internal state; and to assume that it must be is a fallacy.

Finally, there is the distinction between *enabling conditions*, the satisfaction of which ensures that a thing or material is in a state of readiness, and *stimulus conditions* which bring about a response, on condition that a thing or material is in a state of readiness. The satisfaction of the intrinsic enabling conditions ensures that a thing or material is of the right nature for the exercise of a certain power.

Generally speaking, the intrinsic enabling conditions are satisfied by a thing or material being of a certain constitution, for example, having a certain chemical composition, or having a certain crystalline structure, or being in a certain state, say having a certain electric charge, and relative to the exercise of any powers the thing or material may have, the possession of such constitutions or states is permanent, that is, they endure. It is partly for this reason that I am suggesting that they should be picked out and assembled as the nature of the thing or material in question. These distinctions are of rather general application. To use the example cited by Ayers (1968, Ch. 5, pp. 80–9, 95-101), the presence of an engine is an intrinsic enabling condition for a car to have the power to move, and must be included in the specification of what it is to be a proper car. The presence of the driver is an extrinsic enabling condition, while the actions performed by the driver to set the car in motion are extrinsic stimulus conditions. All of these conditions are internal to the car.

To ascribe a power to a thing is to locate the thing or material, for the purposes of understanding its behaviour, in certain contexts of action along towards the agent end of the spectrum of paradigms of action, and to remove it from the patient end. At the patient end, things and materials do not have powers, because at that end of the spectrum of action, effects are brought about through the agency of extrinsic stimuli or conditions.

'Power' is a notion particularly associated with agency, with the initiation of trains of events, with activity, and though philosophers in the past used to speak of passive powers, it seems better to use a quite different term for such attributions as imply that the thing or material falls under the Helmontian paradigm. I want to maintain the everyday connection of the concept of power with that of activity of initiation, of cause and so on, though *not* anthropomorphically, but through the distinctions between the degree to which intrinsic nature and extrinsic circumstances enter into an explanation of the behaviour referred to in the hypothetical component of a power ascription. I shall use the term 'liability' for a 'passive power'. The failure to keep powers and liabilities distinct was another failure of the disposition theory. Most things and materials not only have powers, but also have liabilities.

The structure of the concept of liability has to be given in terms of dispositions and natures, like the analysis above of the concept of power, but now a thing's or material's liabilities are its dispositions to suffer change in virtue of its essential nature—that is the stimulus which produces the change is part of the extrinsic circumstances. When we say of someone that he is liable to catch cold, what we mean is that in the extrinsic circumstance of infection he has a disposition to develop a cold in virtue of his essential nature. We are speaking in a similar way when we talk of someone as having a delicate constitution. Many properties of substances are strictly speaking liabilities and not powers, though their analysans is formally alike; e.g. all those old favourites like 'solubility', 'inflammability', 'brittleness', 'magnetic', etc. In the days when the picture of nature as a crowd of passive sufferers of external and imposed causality was in vogue, these were the only dispositional properties that ever got mentioned. But these only have a role where the world is also full of things and materials with powers. Nothing could be brittle in a world where nothing could smash, nothing could be inflammable in a world where nothing had the power to ignite and there could be no solutes where there were no solvents.

Not only were the analyses of such terms as 'brittle' defective in failing to acknowledge the role of the essential nature of a material or thing, but they were grossly misleading in their direction of attention to the sufferings and passive pronenesses of things, leaving out of account the powers and agencies of materials and individuals, i.e. they led one to overlook the role of the intrinsic natures and over-emphasise the role of the external circumstances. I want to emphasise that the concepts of power and liability, as I have distinguished them, are the poles of a spectrum of concepts, distinguished by the relative degree to which we assign responsibility for particular behaviour between intrinsic conditions and external circumstances. I am not sure that any naturally occurring entity can fall under the pure concept of a patient, since it must be qualitatively identifiable to be known to exist, and hence must have at least the power to manifest those qualities. To ascribe a power is to say that when any of a certain specific kind of phenomenon occurs the intrinsic nature of the things and materials involved are in more or less measure responsible for that phenomenon occurring.

It follows from this account that it is always incorrect to analyse an expression used to attribute a power or a liability wholly in terms of hypotheticals. That there have sometimes been superficially satisfactory analyses of power expressions in terms of hypotheticals is a consequence of the fact that in these analyses there has been a subjunctive component. The basis of our acceptance of 'would have' and 'could do' statements is just that which differentiates power ascriptions from quality ascriptions, namely that those things to which powers are ascribed are of such a nature or constitution that that is the way they *must* behave. Our theory of natural necessity works well in this context, since in assigning part responsibility to the nature of the thing or material we are offering a reason, independent of the actual facts, why those facts are as they are. Similarly, if law statements are offered in part analysis of statements ascribing powers, their nomological character derives from the tacit embedding of such statements in a view about the responsibility of the nature of the things or materials in producing the phenomena with which the law is concerned. Within this view one may see their behaviour as flowing from their natures or constitutions as consequences of what they *are*. So they must behave in the specified way, or not be the things that they are. And so necessarily while they are the things they are, they behave in those ways or have a tendency to behave in those ways or are disposed to behave in those ways mentioned in the hypothetical statement. Being of the right nature endows a thing or material with the power to manifest itself in certain ways or to behave in certain ways in the appropriate circumstances. In the extreme case of the pure agent, it is to do something or to be capable of doing something whatever the circumstances, i.e. to be the initiator of an act. A pure agent is that thing or material in which a causal chain terminates, be it the resolute gardener or a radium atom. I doubt if there are really any pure agents, either organic or inorganic.

At this point nothing has yet been said about the meaning of the expressions 'in virtue of having a certain nature', 'flowing from the nature or constitution of a thing or material' or 'being endowed with a power', or of 'the nature of a thing being responsible for certain phenomena occurring'.

A very important consequence indeed follows too from all this: a consequence of critical importance for the epistemology and logic of scientific discourse and knowledge. If the patterns of phenomena are explained by reference to the powers of the things and materials concerned, and sentences used to ascribe these powers are analysed into a conditional clause expressing the hypothetical and a categorical clause, referring to the natures of things and materials, then because the latter clause indicates that there is a reason why the predicates characterising a thing or material are found together, power talk always involves sets of non-independent predicates.

THE EPISTEMOLOGY OF POWERS

Before we can discuss the criteria for deciding upon the truth or falsity of ascriptions of powers we must face the objection that has been tacitly or explicitly accepted by most philosophers at least since Hume. It is that a sound, empiricist, no nonsense philosophy, the kind of philosophy which science ought to have, must not employ such concepts as 'power', since these introduce bad and occult characteristics which science, with its emphasis on ascertained fact and observation, has been struggling to banish from our view of the world. The alleged absurdity of offering a *virtus dormitiva* as an explanation of the fact that taking opium leads to sleep is so taken for granted as to allow that very example to be trotted out in elementary philosophy books as an instance of fatuous pseudo-explanation. It is then supposed that this is to be replaced by the allegedly scientific explanation that this man has been put to sleep by opium because all cases or most cases of opium taking have been followed by sleep, and the mind anticipates the senses and expects further cases of ingestion of opium to be followed by drowsiness. But with our analysis of power up our sleeves we can turn the tables here. In fact, the Humean emptiness of the general concomitance statement contrasts pretty unfavourably with the *promise* of the power statement, which says that the cause of the sleepiness is not to be put down just to the fact of the ingestion of opium but is to be looked for in the nature of opium itself. This is important because two different sorts of empirical investigation branch off from here. Following Hume's account of the affair the man who wanted to know more could only proceed by collecting more cases of that and similar phenomena until he had enough to convince himself of the lawfulness of the statement of concomitance. He who ascribes the effect to a power of opium does not collect further statistics but begins an exhaustive analysis of opium trying to find what sort of stuff it is, what is its chemical constitution. Then he tries to follow it through the body after ingestion. and tries to ascertain how it acts upon the central nervous system, the higher centres, and so on. He undertakes quite a different sort of investigation. *He does what scientists actually do.* Powers are not occult qualities at all, since they are not qualities. To ascribe them is to offer schematic explanations, and in the growth of knowledge of the natures and constitutions of things the explanations are filled out.

Things and materials are in fact structures of other things and materials. So the analysis of common salt into a cubical lattice of sodium and chloride ions, of steel into iron, iron carbide and carbon provides us with some idea of its nature, in virtue of which it is, for example, soluble. The nature of salt in virtue of which it has its powers is that it is a cubical lattice of those ions. In this account, such powers of the ions as their electric charge are used. These must be again subjected to a similar analysis. Since this analysis, in its turn, refers to the nature of sodium and chloride ions it opens an empirical question of a specific account of their natures. Any entities like protons and electrons which might figure in such an account must be assigned powers which have to be similarly analysed, and so on. But things might have been continuous media distinguished at different places by different states of strain. What are field potentials but powers of the field? If there is no way by which the nature of the field in virtue of which such potentials exist can be specified then we have reached what, for the time being, must be an ultimate level of scientific analysis.

We are now in a position to discuss the criteria by which we judge an ascription of a certain power to a thing or material to be true or false.

To ascribe a power is to assert a specific behaviour hypothetical together with an unspecific categorical referring to the nature or constitution of the thing or material concerned. Behavioural hypotheticals are judged by tests and trials, constitutional categoricals by analyses and observations, for instance, under a microscope. To test the ascription of a power some test or trial must be undertaken, because the hypothetical is specific;

but because the categorical is unspecific we do not have to spell out the nature or constitution of the subject in detail, i.e. one is not called upon to perform the analysis of the thing or material to be justified in ascribing a power. In the ascription of powers the categorical component is like a promissory note, we need only believe that it is not logically impossible for it to be cashed. In order for it to be proper to ascribe a power we must believe that it is in principle possible to ascertain the nature of the subject. We are not committed to being able to tell it then and there. Analysis of the subject need not be carried out along with the test in order for us to be able to say that such and such a thing or material has such and such a power, though to ascribe a power is to locate the cause or part of the cause of what something does or can do in the nature of that thing itself.

So the verification of the ascription of a power is a two-stage process. In the first stage a test or trial shows that the specific hypothetical is true; if dynamite is detonated, there is an explosion. In the second stage the nature or constitution of the material is investigated and it is seen that it is in virtue of its chemical nature that the dynamite can explode. The power to explode is properly attributed to the dynamite after the trials have shown that it does go off, on condition that we believe that the explosion is a consequence of the nature of stuff, even though at that stage we may not know what its nature is, i.e. that the intrinsic nature of dynamite is a largish component in the scientific account of the explosion. It was discovered by trial that fresh lime juice had the power to prevent scurvy, long before anything was known of the nature and manner of working of Vitamin C.

Similarly, if I want to rebut the ascription of a power to a thing or material, it may be that it does not even pass the first stage of the test, as when we suppose that some new synthetic may have the power to cure arthritis, but after a trial on some hopeful, but by now cynical hospital inmates, we find that, as has happened only too often in the past, it does not work. Sometimes, however, a trial of something may seem to give the required result and one might say that it has the power because the hypothetical component seems to be true. But, alas, on pursuing the second stage of the investigation, on looking into the nature of the stuff and its manner of working, it turns out that it could not have been responsible, it was not the right sort of stuff. For example, a placebo may pass a test designed for a drug, though the investigators know it is neutral, because it is not the kind of substance, is not of the chemical constitution, for the observed disposition to flow from its nature.

A final epistemological point: the muddle between dispositions and powers derives in part from the fact that statistics of behaviour do provide a prima facie reason for the ascription of a power, and we are properly entitled to ascribe a power on the empirical basis of that sort of fact alone, because we do not have to have *specific* knowledge of the nature or constitution of the stuff before so doing. But knowledge of the natures of things is usually not sufficient grounds for predicting how they will behave, we need empirical trials as well. It is just this point that lies behind the demands of governments that aeroplanes be tested for airworthiness however studiously they have been designed. We shall see later that this itself follows from a quasi-logical feature of the metaphysics of the concept of power, namely the stratification it imposes on scientific knowledge.

POWERS AND NATURES

Powers must be possessed relatively permanently because in attributing a power we are asserting that the behaviour is a consequence of the thing having such and such a nature, that is, we are asserting that this behaviour is not entirely explicable by the stimulus to which the thing or material has been subjected. If powers do depend in this way upon the natures of the things, then since things must continue to possess their natures (to be themselves) whether they are being stimulated in this way or that, they must permanently have their powers. Thus the quiescent stick of dynamite can explode, the packet of arsenic can poison, the carbon dioxide can form hydroxyl ions in solution, the acid can turn the litmus paper red, the loaded gun can go off, the somnolent gardener can spring into action, and what we mean by these remarks is that they are of that nature. Dynamite is an explosive, arsenic is a poison, carbon dioxide is soluble, litmus paper is sensitive to acid, the gun is loaded, and the sleeper is a keen gardener. To say that dynamite is explosive, to go over to the adjectival form, asserts in one short sentence both what it *will* or *could* do and that in explanation of that the intrinsic nature of the things or materials concerned are involved.

At this point it will be useful to contrast 'power' with two somewhat similar notions, that of 'capability' and that of 'tendency'. Each is differently related to the thing or material which possesses them. Powers have been closely related to natures, and a loss of powers or a decline of powers is evidence for a change in nature. Indeed, in some non-scientific contexts the connection between continuity of power and numerical identity is a necessary one. To strip a deputy of his powers is just what is required to make him cease to be a deputy. A hangman without the power of execution is a hangman no longer, he is but a former hangman. After a time calcium chloride loses its power to absorb water, and we are inclined to make this out as a change in nature going so far as to distinguish, for example, in the case of lime, between anhydrous calcium oxide and hydrated lime, treating them as different substances.

Capabilities do not seem to be related so tightly to natures or constitutions. So that loss of capability may be put down to the thing or material now being in pretty unfavourable circumstances for the exercise of them. In non-scientific contexts this is fairly clear, as for instance the use of 'incapable' for those cases where one's nature has not changed but one is either drunk or with a broken leg. In such a case an external circumstance interferes with the normal manifestation of one's nature as a judicious bystander or a ballroom dancer. These ordinary language considerations are useful as indicating that we might have here a family of concepts, ways in which the basic notion of power, as we have developed it, is qualified in various ways for different purposes. So far then, we have loss of power associated with a change of nature and with the beginnings of a loss of identity. This will be studied in more detail below. 'Loss of capability' seems to be a concept whose ascription implies a certain permanence or constancy of nature, since the qualification of power in such cases is put down to interference with its normal manifestation through external circumstances.

'Capability' as a positive concept expresses something of this idea: a capability is a power which can be acquired (or lost) without there being a change in the fundamental nature of the thing or material in question. Such a concept, if we are to be able to bring it into use, requires a consequential qualification of the concept of 'nature' or 'constitution' of a thing or material. Since to acquire a capability is to make some change in one's nature we need the idea of a change of nature which preserves self-identity as compared with a change of nature which destroys self-identity. It is the whole burden of the supplemented dispositional analysis of power that natures and constitutions are neither just the totality of manifested qualities nor the totality of dispositions, which a thing exhibits in its actual behaviour. I shall return to this very important point in a later section.

Examination of the notion of tendency leads to another qualification of the notion of power. A tendency is a power in abeyance, a power in the course of coming to be exercised, a power about to be exercised or manifested. So, and here we have the strong part of the case against Ryle and Hume, powers may be in opposition in such a way that though much is going on, nothing actually happens. The powers of greed are not abrogated in the case of Balaam's Ass, they are exercised, but still nothing happens, and the explanation of the fact that nothing happens is given in terms of the exercise of power.

THE IDENTITY OF POWERS THROUGH TIME

A power can be exercised over and over again or not exercised at all. So the possession of a power is different from the exercise of a power. Provided the constitution or nature in virtue of which the power is possessed remains unimpaired, then some divergence in outcome attributable to divergencies in the conditions under which the power is exercised is compatible with power identity through time. Alcohol retains the power to intoxicate despite the divergent effects it has upon different people, since these can plausibly be attributed to differences in the people, i.e. to circumstances extrinsic to the nature of alcohol. However, one of the species of power is capacity, and we are familiar with the idea of ' impairment of capacities' and even of 'being incapacitated'. These are cases where we wish to say that a power has been lost or diminished through some temporary or permanent change in the *nature* of the individual involved. So the divergence of outcome from normal, or even failure to have the proper outcome at all cannot be unequivocally construed without reference to the identity or loss of identity of the material or individual involved.

Some powers may be diminished or augmented without losing identity. A drug may lose its effectiveness after a while, but this may happen slowly. We may still attribute to it the power to stop pain, but we may want to say that it does this less efficiently. The power of a car engine may decline. A man may slowly lose the power to move his limbs, his capacity to remember poetry, his capability as a teacher. Catalysts lose their power of catalysis. Powers can also retain their identity while they are augmented.

Are there powers which have to be possessed in full or not possessed at all? By this I mean 'Does our concept of power cover such cases?' Well, as we have already noticed, there are cases of legal or constitutional powers where a certain person either has the power to sentence, the power of attorney, etc., or he just hasn't. Then there is the case of the priest, who has the power to forgive sins. While he has it, this power can neither be diminished nor augmented, but is co-terminal with his being a priest. I do think that the way we use the concept of power covers both powers which can diminish or be augmented, and powers which are either had or not had in their entirety. I shall distinguish these as 'variable powers' and 'constant powers'. As always with powers, we must now consider how they are related to the natures in virtue of which they are possessed. Is an invariable nature associated with a constant power? Might we conclude from a belief that the nature of the material has not changed that it has an undiminished power, say to turn litmus paper blue? And is the variability of a power ultimately dependent upon the variability of a nature, and what then is the criterion of identity under variability of that kind of nature? Does our distinction of two kinds of powers indicate a distinction between two kinds of natures, and thus ultimately two kinds of individuals? Are the bearers of constant powers Parmenidean individuals whose natures are unchanging under identity, and are the bearers of variable powers Aristotelian individuals whose identity is compatible with considerable changes in their nature? I am inclined to think that the answers to all these questions are affirmative.

Scientists make use of both kinds of individual, devising concepts for ultimate individuals that are, as far as possible, Parmenidean. But through being forced to acknowledge the increasing fragility of the structure of things and substances from tough atoms, through fairly robust molecules to fragile crystals and delicate organisms, there is a tendency to devise concepts for more complex individuals that are Aristotelian. So we should expect ultimate individuals to have their full powers while they retain their identities, that is, while they exist at all. These powers could neither be diminished nor augmented without a change in the nature of the individuals who have them. This is reflected, for instance, in the use of positive and negative charge, as a classificatory concept for distinguishing *kinds* of ultimate individuals. While the electron is playing the role of an ultimate entity, it is that individual which has a constant elementary negative charge, i.e. a permanent and unchanging power of a certain kind. It is a necessary condition for an individual to be ultimate that it permanently possesses constant powers, and a sufficient condition for an individual to be not ultimate that it possesses variable powers, but it is not a sufficient condition for an individual to be ultimate that it possesses constant powers. The chemical elements, though they are ultimate for chemistry, are not ultimate absolutely, though their powers, i.e. valencies, are constant.

IDENTITY OF NATURE AND MULTIPLICITY OF POWERS

We say that the same electronic constitution confers two distinct powers upon chemical atoms: the power of entering into certain chemical combinations only, and the power to radiate a particular spectrum. Here difference of power depends upon difference of disposition, in virtue of the same nature. So in this case identity of power seems to rest upon identity of disposition and not upon identity of nature. Similarly, if two materials exhibited the same disposition but in virtue of different natures our concept of power is such that we would say they had the same power. The power to produce a certain electro-motive force may derive from a device's having a certain chemical constitution, like an electrolytic cell, or from a device's having a certain physical constitution, like an electromagnetic generator. It is a contingent, albeit *very* pervasive fact that the same power is usually found with the same nature. It is this important fact that allows the separate empirical study of dispositions and of natures, and the two stage processes of verification and falsification of power-ascriptions.

For those things which have shown their powers under our observation and lapsed back into quiescence again, we may assert the identity of the power on the assumption that the essential nature of the things or stuff has not been changed by the stimulus and reaction to it. But once a piece of paper has burned, it is no longer inflammable, so a piece of paper ash no longer has the nature of paper, and so no longer has the power to burn. But provided that we do have reason to think that the thing or stuff will respond to stimuli in a like manner again, then we may say it has the same power or powers. What would provide us with such a reason? Surely our belief that the essential nature of the stuff had not changed. So our belief in the identity of a power through time depends upon an assumption of the identity of an essential nature through time. And in many cases we can empirically determine whether the essential nature in virtue of which the power is possessed has persisted, without evoking the power. There are certain signs which tell an experienced miner whether the dynamite has so changed its character that it will no longer explode or in hot conditions will explode spontaneously. But as we have seen, the fact that the essential nature may confer several different powers upon a thing or material prevents us from *identifying* the ascription of a power with the identification of an essential nature, though as we have just seen, we may ascribe identity to whatever powers are conferred by the possession of a certain nature on the basis of the identity of that nature through time.

I now come to a more complex case both for the theory of powers and for the problem of identity. So far we have seen the identification of a power to depend upon the persistence of a disposition, and our belief that a thing will continue to behave in a like fashion is backed up by identity of nature as a reason for the identity of this power through time. But there are cases where we speak of different manifestations of the same power, that is where what a thing or material does and can do, shows a wider diversity than the powers we ascribe to it. If this is the case, it will follow that we cannot pass in any simple-minded way from the observation of specific kinds of behaviour to the ascription of specific dispositions, to specific ascriptions of power in all cases. Oxygen has a chemical valency of two, i.e. it has a certain power to combine in certain specific ways with other chemical elements. This power is manifested in different ways on different occasions, depending upon the chemical situation. It is the power manifested in the combination of one atom of oxygen with two atoms of hydrogen, *and* in the combination of two atoms of oxygen with one atom of carbon. An electron manifests its elementary electric charge in a great variety of ways. We could hardly make sense of these cases if we identified powers with dispositions. Hydrogen is manifesting the same power in forming diatomic molecules in the gaseous state as it is in combining in the proportions two-to-one by volume with oxygen. Its disposition to form diatomic molecules, and its disposition to combine with oxygen are different dispositions. The power to which we refer these kinds of behaviour we call its valency. It makes better sense, I maintain, to speak in this way rather than to speak of its power of self-combination and its power to combine with oxygen, and so on, because we refer these dispositions to a common nature, that is to the structure of hydrogen as a simple one electron, one proton structure, and this is its *essential* nature, that is, it enters into the account of all its dispositions. Notice too that hydrogen and indeed all the chemical elements have something of the character of natural kinds. Their structure is supposed to be such that their differences are discontinuous. All their powers are consequences of their intrinsic natures. In these cases it is the common nature and not the identity of dispositions that makes the running in the assertion of the identity of powers.

THE ROLE OF THE CONCEPT OF POWER IN A METAPHYSICS FOR A REALISTIC VIEW OF SCIENCE

There are two places where a realistic point of view in the philosophy of science seems particularly to require the concept of 'power'. These two places are (1) in explanation and causality, and (2) in observation and description. A satisfactory non-Humean and realistic account of causality, of explanation, and so on, can be achieved with the help of the idea of a causal mechanism, and this leads on to the requirement that causal knowledge and indeed knowledge proper must be stratified (Harré, 1964). Unless a new concept could be found, the regress of strata of explanation would either be endless, or if it were terminated (and in practice it must terminate), it would have to terminate in a species of interaction which was of the regular sequence and Humean type and we would be no further forward, having pushed the irrational back a few steps but not scotched it. Though incorporating *ad hoc* methodological recommendations to keep on trying can bring some comfort, it is obviously an unsatisfactory way to deal with the problem.

A regress of explanation is closed, in a proper way, by adverting to entities, individuals, and materials, which are characterised solely by their powers; that is, specifically by what they can do, which we do know, and only unspecifically by what they are, which for the entities which close regresses of explanation, we do not know. To ascribe a power, to characterise by powers, is to *open the question of the nature of things, without being obliged to answer it.* The ultimates can be bare powers, indeed for rational discourse they must be bare powers since to say that some thing or material is of an ultimate kind is to refuse, at least for the moment, to offer any account of its intrinsic nature.

But characterising entities by their powers does not close off all possible further increments of knowledge. We would not have been wrong to ascribe ultimate powers if later the natures of such entities in virtue of which they had the powers were to come to light. However, were we to operate still with the old idea of trying to characterise individuals and materials by their qualities we should be in a very difficult position indeed with respect to the ultimate entities of a regress. To say what qualities things have is to say what they are and then from what they are we are supposed to be able to infer what they do. Qualitative talk draws us irresistibly towards the attempt to define individuals and materials as complexes of qualities, with all the attendant difficulties which I need not rehearse and with all the temptations to fall back into a first substance theory, even if it be disguised by such talk as of 'logical subjects', not to mention finding oneself with Humean nomological concomitance in the end. Even in the end the idea is incoherent. All this has been said once and been forgotten. This is the doctrine of Boscovich in *A Theory of Natural Philosophy*, and of Kant in *The Metaphysical Foundations of Natural Science.* We can now see, from yet another point of view, why the conceiving of iconic models is the central move in the dynamics of rational theory construction, since iconic models stand in for the real structures which are the real natures and constitutions of the things and materials of the world in virtue of which they have their powers. To discover how a thing or material behaves is to discover its powers, but since this leaves its nature and constitution undisclosed, it is not enough. Scientific knowledge also consists in the knowledge of the natures of things in virtue of which they have the powers they do.

POWERS AND QUALITIES

(For what follows, c.f. also Joske (1967) Chs. 4, 5). What do we mean when we say that something or other is yellow? In unreflective and nonscientific contexts we certainly intend to say what hue it is, and we say this on the basis of what hue it manifests to us. But if that were what was meant, then 'yellow' and words like it could not, it seems, form part of a many-person vocabulary, and perhaps could form no part of any vocabulary, since it is logically possible that the same object may manifest itself differently to each of us, and perhaps differently to any one of us on different occasions. 'Yellow' *is* part of a many-person vocabulary, and the apparent contradiction might be resolved by maintaining that the colour word is used to locate the hue manifested to each of us in a system of hues, and that systematic isomorphism is enough for a publicly usable vocabulary to exist. I know of no objections to this view for ordinary description and identification of things and materials. But it certainly will not do for sophisticated and scientific contexts.

Case 1: *A Sophisticated Context.* Seeing something red under sodium light, one says to one's naive companion, 'That may look black, but it's really red'. It is customary to construe 'This is really red' as meaning either 'This would look red under normal conditions' or perhaps 'This would reflect light of 8,000 Å only none is now falling on it'. It is important to see that both these statements are attributions of powers and not attributions of qualities. To say of a thing that it is really red is, in such contexts as these, to say that it has the power to manifest itself as of a certain hue in the appropriate conditions. We usually find out that things have such powers by their manifesting themselves in the appropriate guise to us. But this is not always true. I might have unwrapped a packet of photo-paper in the darkroom, and, on seeing that it looked red, have known that it was really white, and then burnt it, without ever having seen it look white. A great deal of experience is presupposed in such an epistemological divertissement. It is clear that to use colour words in such contexts in such ways is to treat ascriptions of colours as ascriptions of powers and not as ascriptions of hues.

Case 2: *The Scientific Context.* What is really going on from a semantic point of view when we start talking about red light, red surfaces, and what not, having in mind the wavelength of light? What has happened to the word 'red'? It certainly no longer is being used to ascribe a quality, a hue, which an observer might observe. Red light cannot be seen at all. If the word meant a hue, then red light would surely be a nonsensical mess of words. 'Red light' means 'light of wavelength 8,000 Å'. The connection is this: light of this nature has the power to affect us in such a way that we experience a certain hue, qualified as in our discussion of the unsophisticated context. The light has this power whether or not it affects a human being. The colour word can be used metaphorically for wavelength, because it is in virtue of its wavelength that light affects us colour-specifically. Similarly, a body has a temperature even when there is no thermometer leaning up against it, and it is then *hot*. What is it that it then is? It continues to possess the power to affect a thermometer in virtue of the state it is in. To be capable of ascribing the power we do not need to be able to specify the state which is responsible, only to locate the responsibility for raising the level of the mercury as intrinsic to the stuff to which we attribute the temperature. In this case, internal and intrinsic states are extensionally equivalent.

All words like 'hot', bright', 'green', 'heavy', 'smooth' and so on, when used by scientists within a scientific context are not only power words, but they have something of the character of metaphors. In the first stage of preparation for scientific use, the word ceases to be used to ascribe a quality and is used to ascribe a power. This attributes a certain nature to the entity involved as subject, but at this stage the verification of the power statement is through the checking of the disposition to look this or that hue. At this stage there must be some situation in which whatever it is really does look red. In the second stage the nature is better known and so can be more exactly specified. We may speak of 'redness' in contexts when that nature is engaged, but not in such a way that a hue can be manifested to anyone, for example in making a numerical measure in a spectrometer. And then we have certainly passed from literal to metaphorical language.

THE LOGICAL FORM OF POWER ASCRIPTIONS

Having groomed our everyday concept of power for a metaphysical role, i.e. as a contender for the place of the traditional concept of quality, and having shown in rather brief outline how the fulfilment of that role might function in the philosophy of science, I want to round off this paper with some consequential topics.

It is incumbent upon one to offer some kind of account in terms of the traditional logic of quantifiers, variables and predicates, of the structure of sentences which might be used to ascribe a power.

We have seen that a power ascription has two components, since it is used to ascribe a specific disposition, and to ascribe some nature or other in virtue of which the thing or material has that disposition. Let us call the thing or the sample of material to which the power is being ascribed '*a*', and the circumstances in which it exercises its power '*C*', noting that *C* need not have occurred for it to be true that *a* has the power. The power is the power to *G*, so *G* is what does happen or might be expected to happen when *a* is in *C*, or were *a* to be in *C*.

As I understand a subjunctive conditional it is a straightforward statement about possibilities, excluding those actualised. So it is just right for part of the analysis of a power statement, since to say that a thing has a power is to say what is possible for it, for that is what it is to talk of its dispositions.

To ascribe a power is also to say that the thing or material has a nature such that these possibilities are open to it. But it is precisely not to specify that nature. It is to say that there is a description of it incorporating the intrinsic, enabling conditions for the disposition to be possible, but it is not to give that description. It is to say that: There is a ϕ, such that something has ϕ, and whatever had ϕ in *C*, *would have to G.*, i.e. if something like *a* did not have ϕ in *C* it would not, indeed *could* not *G*.

What about our reasons for accepting this? It is a hypothesis. It is not a law of nature because at the stage that we are ascribing powers we may or may not know what description ϕ is, or even if such a description can be found. It should be perfectly clear from this rather unsatisfactory formula, if from nothing else, that to ascribe a power is to leave the possibility of further discovery open, and in particular to leave open the discovery of which ϕ is true of *a*. There are further issues here, namely, the force of the modal operation, and these raise epistemological questions of interest. This rough analysis does not, of course, allow powers to be defined out as logical functions of qualities, since by the earlier argument I offer a powers analysis of observations, so the predicates ϕ and *C* in this formula also stand for powers. This, I am encouraged to find, is in line with Joskes' treatment of perceptual qualities in his superb *Material Objects*.

Science, then, conceived as the study of the behaviour of things and materials, and the elucidation of their natures, and so of their powers, prompts the investigation of natures and the analysis of natures leads to the discovery of new kinds of things and materials to which again powers are first ascribed, restarting the cycle.

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Induction, Reason and Consistency

by KEITH LEHRER

THE PHILOSOPHICAL CREDENTIALS of inductive logic are controversial. Some philosophers aver that inductive inference is a central feature of rational justification and explanation. Others doubt the epistemic legitimacy of such inference. One defence of inductive logic consists of formulating a rule of inductive inference that avoids paradox and increases philosophical understanding. To this end, I shall propose an inductive rule that avoids a well known paradox, the paradox of the lottery, and that is demonstrably consistent, conjunctive, and deductively closed. Finally, I shall argue that the rule is both a canon of rationality and a principle of explanation.

1 THERE ARE two important kinds of inductive rules. The first are rules for ascertaining probability relations among statements. Such rules have been proposed, and I shall not add to the collection in this paper. I shall instead be concerned with a second kind of inductive rule specifying when a statement h may be induced (inductively inferred) from another statement e once the probabilities among statements have been ascertained. The formulation of a rule of this second kind may guide the choice of a rule of the first kind as well as the other way round, and the rule of induction I propose has definite implications for the choice of a rule for ascertaining probabilities. I am assuming that just as one concern of deductive logic is to specify when a statement may be deduced from another, so one concern of inductive logic is to specify when a statement may be induced from another. The formulation of a satisfactory inductive rule of the second kind is, I believe, an indirect justification of this assumption.

2 ASSUME THAT probability relations among statements of some finite language have been ascertained in accordance with a usual calculus of probability. In such a language, when may a statement h be induced from another statement e? It is natural to suppose that there is some degree of probability less than unity that is high enough to insure inducibility and such that no lower degree will suffice. In that case, letting '$\mathrm{I}(h,e)$' mean 'h may be induced from e', there is some pair of positive integers m and n ($m<n$) such that we could adopt the following inductive rule:

R1. $\mathrm{I}(h,e)$ if and only if $\mathrm{P}(h,e) \geq m/n$.

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However, it also seems natural to require that the set of statements that may be induced from e should be consistent with e provided e itself is consistent. Though induction need not preserve truth, and we expect occasionally to induce a false conclusion from a true premise, induction should preserve consistency, i.e. we do not expect to induce conclusions inconsistent with a consistent premise. Thus, we should adopt the following consistency condition:

CC. The set S_e of all statements h such that $I(h,e)$ is consistent with e. That is, it is not the case that $S_e \vdash \sim e$.

But R1 and CC lead to contradiction.

The contradiction is referred to as 'the lottery paradox'. Where m and n are as in R1, suppose we have a lottery of n tickets, consecutively numbered starting with one, and the evidence that exactly one of the tickets has been drawn. Letting e be a statement formulating this evidence, consider a set of hypotheses h_1, $h_2 \ldots h_h$, where h_1 says that the number one ticket has been picked, h_2 says that the number two ticket has been picked and so forth. Since m and n are positive integers, and assuming that each ticket is as likely to be picked, as any other, we can conclude that $P(\sim h_1,e) \geqq m/n$, $P(\sim h_2,e) \geqq m/n \ldots$ and $P(\sim h_n,e) \geqq m/n$. It follows from R1 that $I(\sim h_1,e)$, $I(\sim h_2,e) \ldots$ and $I(\sim h_n,e)$. If we consider the disjunction D_h where $D_h = h_1 \vee h_2 \ldots \vee h_n$, it is obvious that $P(D_h,e) = 1$. Thus, $I(D_h,e)$ follows from R1. Therefore, the set of S_e of hypotheses h such that $I(h,e)$ contains $\sim h_1$, $\sim h_2$, $\ldots$ and $\sim h_n$ in addition to D_h. This set of hypotheses is inconsistent with e. This proves the inconsistency of R1 and CC.[1]

3 To ESCAPE from this contradiction, we must reject either R1 or CC. It is most reasonable to reject R1. The condition CC only asserts that the hypotheses induced from our evidence must not be logically inconsistent with the evidence, and surely that is a reasonable requirement. One consideration that might appear to favour another conclusion is the fact that men do reach inconsistent conclusions starting from the same evidence. But this is a simple result of the fact that some men reach conclusions they are not entitled to reach. Sometimes a man infers h from e because he mistakenly thinks that h can be deduced from e when it cannot. Similarly, sometimes a man infers h from e because he mistakenly thinks h can be induced from e when it cannot. Thus, the fact that men infer contradictory conclusions from the same evidence shows merely that at least one has made a mistake. The conclusion *he* drew cannot be induced from the evidence.

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[1] The lottery paradox was first formulated to my knowledge, by Kyburg (1961), p. 197. Kyburg (1965) suggested dropping the consistency condition, which was advocated by Hempel (1962), p. 151.

I conclude that we must reject R1. Moreover, it will not help to weaken R1 as follows:

$$\text{R}^{*}.\quad \text{If } P(h,e) \geqq m/n, \text{ then } I(h,e).$$

The same contradiction may be derived from R*. Indeed, the foregoing contradiction assumed no more than R*. Thus, no degree of probability less than unity ensures that a hypothesis *h* may be induced from evidence *e*. Should we conclude that no hypothesis may be induced from any evidence when the probability of the hypothesis on that evidence is less than one?

Such a conclusion would be unreasonable. First, Hintikka and Hilpinen (1966) have shown that, if we restrict our rule to a certain kind of hypothesis, we can formulate a rule that will enable us to induce such hypotheses from the evidence without inconsistency when the hypothesis has a probability of less than unity on the evidence. Secondly, Levi (1967) has shown that if we restrict our rule to a given finite partition we can again induce with a probability less than one without arriving at any inconsistency. Let us consider what sort of unrestricted rule we may substitute for R1, retaining CC, while avoiding the lottery paradox.

4 THE LOTTERY PARADOX has three central features that should be noticed. The first is that the hypotheses $h_1, h_2, \ldots h_n$ are logically exclusive in pairs and logically exhaustive as a set relative to evidence *e*, that is, for any h_j and h_k $(j \neq k)$, $e \vdash \sim(h_j \& h_k)$ and $e \vdash (h_1 \vee h_2 \ldots \vee h_n)$. Secondly, for any h_j and h_k, $P(h_j,e) = P(h_k,e)$. Thirdly, as a result of the first two features, the set of hypotheses $\sim h_1, \sim h_2 \ldots \sim h_n$ are logically exhaustive in pairs and logically inconsistent as a set relative to *e*, that is, for any h_j and h_k $(j \neq k)$, $e \vdash (\sim h_j \vee \sim h_k)$ and $e \vdash \sim(\sim h_1 \& \sim h_2 \ldots \& \sim h_n)$. Also, $P(\sim h_j,e) = P(\sim h_k,e)$.

The solution to the problem depends on the realisation that the hypotheses, $\sim h_1, \sim h_2 \ldots \sim h_n$, are competing hypotheses relative to evidence *e*. It is obvious that the hypotheses h_1 and $\sim h_1$ compete relative to *e* because they cannot both be true. It is equally obvious that the hypotheses $h_1, h_2, \ldots h_n$ are competitive. It is less obvious that the set of hypotheses $\sim h_1, \sim h_2, \ldots \sim h_n$ are competitive, but they are. Rule R1 required that an induced hypothesis be more probable on the evidence than any hypothesis with which it is inconsistent relative to the evidence. But, the lottery paradox shows that a hypothesis competes with hypotheses with which it is perfectly consistent relative to the evidence, for example $\sim h_1$ competes with $\sim h_2$. How should we characterise the relation between $\sim h_1$ and $\sim h_2$? There are two principal characteristics. One is that they are subcontraries relative to e, that is, $e \vdash (\sim h_1 \vee \sim h_2)$. Another is that they are not deductive consequences of e, that is, it is neither the case that $e \vdash \sim h_1$ nor that $e \vdash \sim h_2$. Hence we may conclude that a hypothesis h not deducible from e is inducible if h is (i) more probable than those hypotheses with which it is inconsistent relative to e and (ii) more probable than those hypotheses which are subcontraries of it relative to e.

Let us restrict out attention to finite language (in which $P(h,e) = 1$ if and only if $e \vdash h$) and consider a more formal statement of the rule. The rule would then be as follows:

R2. $I(k,e)$ if
(i2a) for any h, if $e \vdash (\sim h \vee \sim k)$, then $P(k,e) > P(h,e)$, and
(ii2a) for any h, if $e \vdash (h \vee k)$ and it is not the case that $e \vdash h$, then $P(k,e) > P(h,e)$.

Unlike R1, R2 is such that none of the hypotheses $\sim h_1, \sim h_2, \ldots \sim h_n$ may be induced from e by appeal to R2. For each of them is as probable as the others, and hence condition (ii) is not satisfied. Consider $\sim h_1$. We know $e \vdash (\sim h_1 \vee \sim h_2)$ and it is not the case that $e \vdash \sim h_1$. But $P(\sim h_1,e) = P(\sim h_2,e)$. Thus, from R2 we cannot generate the lottery paradox. Moreover, if we can prove that $I(h,e)$ and $I(k,e)$ by appeal to R2, then either $(e \& h) \vdash k$ or $(e \& k) \vdash h$. From this it follows that either $P(k,e\&h) = 1$ or $P(h,e\&k) = 1$ by the calculus of probability. This makes it possible to prove that the set of hypotheses induced from e by appeal to R2 is consistent with e.

However, rather than offer a proof of this, it will be useful to consider an equivalent rule that is simpler. It turns out that if a hypothesis competes with those hypotheses inconsistent with it and with those of its sub-contraries not deducible from the evidence, then it competes with every statement not deducible from the original hypothesis conjoined to the evidence. More precisely, we can show that $I(k,e)$ by appeal to R2, if and only if we can show that $I(k,e)$ by appeal to a rule that requires that $P(k,e) > P(h,e)$ for any h that is not a deductive consequence of k relative to e. Basically the reason is this. Suppose h is logically independent of k relative to e. Nonetheless, the disjunction $\sim k \vee h$ is a subcontrary of k relative to e, that is, $e \vdash k \vee (\sim k \vee h)$. Hence, if we suppose that the disjunction $\sim k \vee h$ is not deducible from e, then $I(k,e)$ may be established by appeal to R2 only if $P(k,e) > P(\sim k \vee h,e)$. However, given the calculus of probability this implies that $P(k,e) > P(h,e)$. Thus we may replace R2 with a simpler rule. The rule is as follows:

R3. $I(k,e)$ if (i) it is not the case that $e \vdash \sim k$, and, (ii) for any h, if it is not the case that $e \& k \vdash h$, then $P(k,e) > P(h,e)$.

It can be proved that for any h and e, $I(h,e)$ by R2 if and only if $I(h,e)$ by R3. (See Appendix, Proof 1.)

Having proved the equivalence of R2 and R3, we shall now prove that R3 satisfies CC, that is, that the set of hypotheses that may be induced from evidence by appeal to R3 are logically consistent with the evidence. We shall prove this by first proving that if two hypotheses may be induced from evidence by R3, then the conjunction of those hypotheses may also be induced from the evidence. (See Appendix, Proof 2.)

Having proved that if $I(k,e)$ and $I(h,e)$, then $I(k\&h,e)$, we shall now prove that the members of a set S_e containing all the hypotheses induced from e by R3 is logically consistent with e. (See Appendix, Proof 3.)

Of course, from the preceding proof it is a corollary that S_e is itself consistent, because, if S_e were inconsistent, then it would be inconsistent with e. Thus, R3 solves the lottery paradox and satisfies the consistency condition.

5 ONCE IT IS UNDERSTOOD that R3 is sufficiently restrictive to avoid the lottery paradox, we must next consider whether R3 is too restrictive or whether some less restrictive rule would also avoid the paradox. In order to ensure that the consistency condition is satisfied, we must have a rule that implies that the probability of any conjunction C of the members of S_e is such that $P(C,e)>0$. R3 ensures this result by implying that if $I(k,e)$ and $I(h,e)$, then $I(k\&h,e)$ and, therefore, that $I(C,e)$. This guarantees that $P(C,e)>1/2$. We do not need to guarantee that $P(C,e)>1/2$ to ensure that $P(C,e)>0$. So a less restrictive rule is feasible. Such a rule can be built upon R3.

Once again let us consider a conjunction C of the members of S_e. Having induced C from e, may we employ C as evidence to induce additional conclusions by using R3, and having induced such conclusions, proceed again in the same manner? The answer is affirmative. This method of adding results can be embodied in a rule that satisfies the consistency condition. To formulate such a rule, let us rewrite R3 as a rule of *direct* induction. We introduce the expression '$D(h,e)$' meaning 'h may be directly induced from e'. R3 rewritten as a rule of direct induction is as follows:

> RDI. $D(k,e)$ if, it is not the case that $e \vdash \sim k$ and for any h, if it is not the case that $e\&k \vdash h$, then $P(k,e)>P(h,e)$.

Now using '$I(h,e)$', we can formulate our inductive rule as follows:

> IR. $I(h,e)$ if and only if h is a member of a set I_e such that
> $I_e = I_1 \cup I_2 \ldots \cup I_m$.
> I_1 = the set of hypotheses h_j such that $D(h_j,e)$, and letting

C_i be a conjunction of members I_i such that for any s, $C_i \vdash s$ if and only if $I_i \vdash s$,

$I_2 =$ the set of hypotheses h_j such that $D(h_j, C_1\&e)$
$I_3 =$ the set of hypotheses h_j such that $D(h_j, C_2\&e)$
$I_m =$ the set of hypotheses h_j such that $D(h_j, C_{m-1}\&e)$.

We can prove that IR satisfies CC. (See Appendix, Proof 4.)
We can also prove that set or hypotheses induced from e is deductively closed; that is, if $I_e \vdash s$, then s is a member of I_e. (See Appendix, proof 5.)

6 HAVING PROVED that IR is consistent, let us now consider the other features of the rule. The most important question to answer is—what would IR allow us to induce from what? Of course, the answer to this question depends on the assignment of probabilities, for IR defines induction in terms of probability. However, if we restrict our consideration to languages suitable for describing lotteries, we shall be able to see what sort of results the rule yields. Let us return to the lottery situation where we have a language in which we can express the hypotheses, h_1, $h_2 \ldots h_n$ asserting that the number 1, number 2, . . . number n ticket was picked, truth functional combinations of these hypotheses, and nothing else.

Let us take as e the statement that exactly one of hypotheses, h_1, $h_2 \ldots h_n$, is true. What we may induce from e by IR will depend on the probabilities. We shall consider three cases in which we assign the probabilities in different ways.

Case 1. This assignment of probabilities is one such that for any h_j and h_k, $P(h_j,e) = P(h_k,e)$. This is the fair lottery. Given this assignment, the set I_e has only the deductive consequences of e as members.

Case 2. Suppose that there is one 'sticky' lottery ticket, the one numbered n, which is less likely to be picked than any other, and that the other tickets are equally likely to be picked. Hence for any $h_j (j<n)$ and $h_k (k<n)$, $P(h_j,e) = P(h_k,e) \& P(h_n,e) < P(h_j,e)$. In this case, $D({\sim}h_n,e)$, and, hence, $I({\sim}h_n,e)$. But for any hypothesis not a logical consequence of ${\sim}h_n$ and having a probability less than 1 on e, the hypothesis may not be induced. When one hypothesis has a lower probability than any other not entailed by the evidence, we may induce the denial of such a hypothesis from the evidence.

Case 3. Suppose that all the tickets are 'sticky' but no two are equally 'sticky' so that the probability on the evidence of each ticket being picked differs from all the rest. Moreover, suppose that for any h_j and $h_k (j<k)$, $P(h_j,e) > P(h_k,e)$. Thus h_1 is the most probable and h_n the least probable hypothesis on the evidence e. In this case, as in the preceding case, $D({\sim}h_n,e)$ because ${\sim}h_n$ is the most probable hypothesis on evidence e of all those hypotheses not logically implied by e. So by IR, ${\sim}h_n$ is a member of I_1.

However, D($\sim h_{n-1}$, $e\&\sim h_n$) by the same argument. So $\sim h_{n-1}$ is a member of I_2. Similarly, $\sim h_{n-2}$ is a member of I_3, and, h_1 is therefore a member of I_m. Thus I(h_1,e).

7 THESE RESULTS may be defended by considering what is required by perfect rationality. Suppose a man is both perfectly rational and is a perfect logician as well. We shall call him 'PRM'. His limitations is that he is ignorant of everything except logic and probabilities. Now if a man is perfectly rational, then if he induces h_j from e but does not induce h_k from e, then he must have some *reason* for inducing one but not the other. Consider Case 1 above. *If* PRM induces $\sim h_1$ he must also induce $\sim h_2$, $\sim h_3$, and so forth to a set of hypotheses inconsistent with the evidence. This is not rational. Thus, if he induces $\sim h_1$, then there must be some h_j such that he does not induce $\sim h_j$ from e. But, since $\sim h_1$ and $\sim h_j$ are equally probable, he lacks a reason for inducing one and not the other. Consequently, PRM would not induce $\sim h_1$ or $\sim h_j$ for any j. This coincides with our results using IR.

Consider Case 2. There is in this case a reason for inducing $\sim h_n$ rather than $\sim h_1$, $\sim h_2$, and so forth, namely, that it is more probable on e. Thus PRM would induce $\sim h_n$. Having induced $\sim h_n$, PRM would then find himself in a situation analogous to the initial condition in Case 1. That is, there would be no reason to induce any other hypothesis, and consequently, he would induce no further. This corresponds to the results obtained by IR. Finally, in Case 3, PRM would first induce $\sim h_n$ because he has a reason for doing so, namely, that it is more probable than any of the hypotheses $\sim h_1$, $\sim h_2$, and so forth. Having induced $\sim h_n$, he would then have a similar reason for inducing $\sim h_{n-1}$ and so forth until he induces h_1. PRM would conform to IR in all three cases and, consequently, the rule may be regarded as a canon of perfect rationality.[1]

8 ANOTHER ARGUMENT in favour of IR may be found by considering the relation of induction to explanation. An inductive rule should lead us to induce the hypothesis which, if true, would be explained by the premises of the induction. In Case 1, whatever ticket is picked, that fact is not explained by the evidence, and whatever ticket is not picked, that fact is not explained by the evidence either. Suppose ticket 13 is picked. There is no explanation of why ticket 13 was picked rather than 14 or any other. Similarly, there is no explanation of why 14 was not picked. That it is highly probable that 14 would not be picked is no explanation, because it was equally probable that 13 would not be picked.

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[1] Canons of perfect rationality are proposed by Swain (1966), esp. in ch. 4.

In Case 2, the one fact that would be explained by the evidence is that ticket number *n* was not picked. For, it was less probable on the evidence that ticket *n* would be picked than that any other would. In case 3, if it is true that ticket 1 is picked, that would be explained by the evidence, because that was the most probable eventuality. Similarly, that each ticket greater than 1 was not picked is explained by the evidence, because it was more probable that some other ticket would be picked. Thus IR leads to the induction of those hypotheses which, if true, would be explained by the evidence.

9 FINALLY, OUR RESULTS have implications for the other problem of inductive logic, that of ascertaining probabilities. What our lottery examples show is that when we assign ***different*** probabilities to the basic alternatives, IR permits us to make significant inductive inferences from the evidence. On the other hand, when we assign the same probabilities to the basic alternatives, IR permits us to induce nothing that cannot be deduced from the evidence. What this means is that the choice of a probability functor that assigns heterogeneous probabilities to basic alternatives of a language, state descriptions for example, will, when taken in conjunction with IR, permit very strong inductive inference. The choice of a probability functor that assigns homogenous probabilities to such alternatives will not permit genuine inductive inference. In general, the greater the heterogeneity the stronger the inferences. Probability functors which are strongly influenced by the logical features of a language rather than the empirical features of the evidence yield greater homogeneity, while those that favour the empirical features of the evidence yield greater heterogeneity.[1] Thus, IR offers some justification for assigning probabilities according to a functor that yields heterogeneous results because it favours the empirical factor. By so doing, we shall increase the strength of our inductions as well as our explanatory power. Such an inductive logic will lead to conclusions that are both true and explained, if the lottery of nature is skewed rather than random. If nature is random, induction is useless. So nature is unfair.

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APPENDIX

The purpose of this appendix is to present proofs mentioned in the body of the paper. The following calculus of probability will be employed in subsequent proofs, but it is assumed that it is not the case that $e \vdash \sim(e\&h)$.

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[1] The influence of logical and empirical factors on probability functors is explicated by Carnap (1962), p. 568.

P1. $0 \leqslant P(k,e) \leqslant 1$.
P2. If $\vdash(k \equiv k')$ and $\vdash(e \equiv e')$, then $P(k',e') = P(k,e)$.
P3. If $e \vdash k$, then $P(k,e) = 1$.
P4. If $\vdash \sim(k\&k')$, then $P(k \vee k',e) = P(k,e)+P(k',e)$.
P5. $P(k\&h,e) = P(h,e) \times P(k,e\&h)$.

If we assume that the language in question is finite, then we may replace P3 with the following postulate:

P3A. $P(k,e) = 1$ if and only if $e \vdash k$.
T1. $P(k,e)+P(\sim k,e) = 1$.
T2. If $k \vdash k'$, then $P(k,e) \leqslant P(k',e)$.

The rules R2, R3, RDI and IR and the subsequent proofs regarding them are restricted to finite languages and sets. Consequently, P3A will be assumed.

Proof 1: That for any k and e, $I(k,e)$ by R2 if and only if $I(k,e)$ by R3.

A. For any k and e, if $I(k,e)$ by R3, then $I(k,e)$ by R2. This is proved by proving that if (i) and (ii), then (ia) and (iia). If $e \vdash \sim k$ then neither (i), nor (ia) and hence not $I(k,e)$ by R3 or R2. Thus in the proof of A we assume it is not the case that $e \vdash \sim k$.

Subcase A1. If (i), then (ia).

1. $e \vdash \sim h \vee \sim k$, assumption.*
*2. $e \vdash k \supset \sim h$, from 1.
*3. $e\&k \vdash \sim h$, from 2 by Deduction Theorem.
*4. It is not the case that $(k\&e) \vdash h$, from 3, and the assumption that it is not the case that $\vdash \sim(k\&e)$.
5. If $e \vdash \sim h \vee \sim k$, then it is not the case that $(k\&e) \vdash h$.

Subcase A2. If (ii), then (iia)

1. $e \vdash h \vee k$ and it is not the case that $e \vdash h$, assumption.*
*2. $e \vdash \sim k \supset h$, from 1.
*3. $e\&\sim k \vdash h$, from 2 by Deduction Theorem.
*4. It is not the case that $e \vdash h$, from 1.
*5. It is not the case that $k\&e \vdash h$, from 3, 4.
6. If $e \vdash h \vee k$ and it is not the case that $e \vdash h$, then it is not the case that $k\&e \vdash h$.

B. For any k and e, if $I(k,e)$ by R2, then $I(k,e)$ by R3. This is proved by proving that if (ia) and (iia), then (ii).

1. For any h, if $e \vdash \sim h \vee \sim k$, then $P(k,e) > P(h,e)$, (ia).
2. For any h, if it is not the case that $e \vdash h$ and $e \vdash h \vee k$, then $P(k,e) > P(h,e)$, (iia).
3. It is not the case that $e\&k \vdash h$, assumption.*
*4. It is not the case that $e \vdash k \supset h$, from 3, Deduction Theorem.

*5. It is not the case that $e \vdash \sim k \vee h$, from 4.

*6. $e \vdash (\sim k \vee h) \vee k$, Logical Theorem.

*7. It is not the case that $e \vdash \sim k \vee h$ and $e \vdash (\sim k \vee h) \vee k$, from 5 and 6.

*8. If it is not the case that $e \vdash \sim k \vee h$ and $e \vdash (\sim k \vee h) \vee k$, then $P(k,e) > P(\sim k \vee h,e)$, from 2.

*9. $P(k,e) > P(\sim k \vee h,e)$, from 7 and 8.

*10. $h \vdash \sim k \vee h$, Logical Theorem.

*11. $P(\sim k \vee h,e) \geq P(h,e)$, from 10 and T2.

*12. $P(k,e) > P(h,e)$, from 9 to 11.

13. If it is not the case that $e \& k \vdash h$, then $P(k,e) > P(h,e)$.

Proof 2: That if $I(k_1,e)$ and $I(k_2,e)$ by $R3$, then $I(k_1 \& k_2,e)$ by R3.

1. For any h, if it is not the case that $e \& k_1 \vdash h$, then $P(k_1,e) > P(h,e)$ from $I(k_1,e)$ by R3.
2. For any h, if it is not the case that $e \& k_2 \vdash h$, then $P(k_2,e) > P(h,e)$ from $I(k_2,e)$ by R3.
3. It is not the case that $e \& k_1 \vdash k_2$, assumption.*

*4. If it is not the case that $e \& k_1 \vdash k_2$, then $P(k_1,e) > P(k_2,e)$, from 1.

*5. $P(k_1,e) > P(k_2,e)$ from 3 and 4.

*6. If it is not the case that $e \& k_2 \vdash k_1$, then $P(k_2,e) > P(k_1,e)$, from 2.

*7. $P(k_2,e) \ngtr P(k_1,e)$ from 5.

*8. $e \& k_2 \vdash k_1$, from 6 and 7.

9. If it is not the case that $e \& k_1 \vdash k_2$, then $e \& k_2 \vdash k_1$, from 3-8.

10. Either $e \& k_1 \vdash k_2$ or $e \& k_2 \vdash k_1$, from 9.

11. $e \& k_1 \vdash k_2$, assumption.*

*12. $e \& k_1 \& k_2 \vdash h$, assumption.†

*†13. $e \& k_1 \vdash k_2 \supset h$, from 12 by Deduction Theorem.

*†14. $e \& k_1 \vdash h$, from 11 and 13.

*15. If $e \& k_1 \& k_2 \vdash h$, then $e \& k_1 \vdash h$, from 12-14.

*16. If $e \& k_1 \vdash h$, then $e \& k_1 \& k_2 \vdash h$, Logical Theorem.

*17. $e \& k_1 \& k_2 \vdash h$ if and only if $e \& k_1 \vdash h$, from 15, 16.

*18. $P(k_1 \& k_2,e) = P(k_1,e) \times P(k_2, k_1 \& e)$, by P5.

*19. $P(k_2,k_1 \& e) = 1$, from 11 by P3, and not $\vdash \sim(e \& k)$ from $I(k_1,e)$ by R3.

*20. $P(k_1 \& k_2,e) = P(k_1,e)$, from 18 and 19.

*21. $P(k_1 \& k_2,e) > P(h,e)$ if and only if $P(k_1,e) > P(h,e)$ from 20.

22. If $e \& k_1 \vdash k_2$, then, for any h, $e \& k_1 \& k_2 \vdash h$ if and only if $e \& k_1 \vdash h$, and, for any h, $P(k_1 \& k_2,e) > P(h,e)$ if and only if $P(k_1,e) > P(h,e)$, from 11-17-21.

23. If $e \& k_2 \vdash k_1$, then, for any h, $e \& k_2 \& k_1 \vdash h$ if and only if $e \& k_2 \vdash h$, and, for any h, $P(k_2 \& k_1,e)$ $P(h,e)$ if and only if $P(k_2,e) > P(h,e)$, proof same as for 22, by interchanging k_1 and k_2 in steps 11-21.

24. *Either*, for any h, $e\&k_1\&k_2 \vdash h$ if and only if $e\&k_1 \vdash h$, and, for any h, $P(k_1\&k_2,e) > P(h,e)$ if and only if $P(k_1,e) > P(h,e)$, *or*, for any h, $e\&k_1\&k_2 \vdash h$ if and only if $e\&k_2 \vdash h$, and, for any h, $P(k_1\&k_2,e) > P(h,e)$ if and only if $P(k_2,e) > P(h,e)$ from 10, 22, 23.

25. For any h, if it is not the case that $e\&k_1\&k_2 \vdash h$, then $P(k_1\&k_2,e) > P(h,e)$, from 1, 2, 24.

26. It is not the case that $e \vdash \sim k_1$ from $I(k_1,e)$ by R3.

27. It is not the case that $e \vdash \sim k_2$ from $I(k_2,e)$ by R3.

28. It is not the case that $e \vdash \sim (k_1\&k_2)$ from 26, 27, and 9.

29. $I(k_1\&k_2,e)$ from 25 and 28 by R3.

Proof 3: That the set S_e of hypotheses h such that $I(h,e)$ by R3 is *not* such that $S_e \vdash \sim e$.

1. If $S_e \vdash \sim e$, then there is a conjunction C of members of S_e such that $C \vdash \sim e$. Logical Theorem.

2. $I(C,e)$, from 1 and Proof 2.

3. $P(C,e) > P(\sim C,e)$, from 2 and R3.

4. $P(C,e) > 1/2$, from 3 and T1.

5. It is not the case that $e \vdash \sim C$, from 4 by P3.

6. It is not the case that $C \vdash \sim e$, from 5.

7. It is not the case that $S_e \vdash \sim e$, 6 and 1.

Corollary of Proof 3: That S_e is not such that $S_e \vdash e\&\sim e$.

Proof 4: That the set I_e such that $I_e = I_1 \cup I_2 \ldots \cup I_m$ is not such that $I_e \vdash \sim e$.

1. It is not the case that $I_1 \vdash \sim e$, by Proof 3.

2. For any I_j $(j > 1)$, it is not the case that $I_j \vdash \sim (C_{j-1}\&e)$, by Proof 3.

3. For any I_j $(j > 1)$, it is not the case that $I_j \vdash \sim e$, from 2.

4. k is a member of I_j, assumption.*

*5. $C_j\&e \vdash k$, from 4 by specification of C_j in IR.

*6. $P(k,C_j\&e) = 1$, from 5 and P3.

*7. For any h, if it is not the case that $C_j\&e\&k \vdash h$, then $P(h,C_j\&e) < 1$, by P3A.

*8. For any h, if it is not the case that $C_j\&e\&k \vdash h$, then $P(k,C_j\&e) > P(h,C_j\&e)$, from 6, 7.

*9. k is a member of I_{j+1}, from 8 and IR.

10. If k is a member of I_j, then k is a member of I_{j+1}, 4-9.

11. $I_e = I_m$, from 10.

12. It is not the case that $I_e \vdash \sim e$ from 2, 3, 11.

Corollary of Proof 4: That it is not the case that $I_e \vdash (e\&\sim e)$.

Proof 5: That if $I_e \vdash s$, then s is a member of I_e.

1. $I_e = I_m$ by proof 4, line 10 and $I_e \vdash s$.

2. $I_m \vdash s$ from 1.

3. s is not a member of I_e, assumption.*

*4. s is not a member I_m, from 1 and 3.

*5. There is a conjunction C_m such that, for any s, $C_m \vdash s$ if and only if $I_m \vdash s$. Logical Theorem.

*6. $C_m \vdash s$ from 2 and 5.

*7. It is not the case that $C_m \& e \vdash \sim s$ from 1 and proof 4.

*8. For any h, if it is not the case that $s \& C_m \& e \vdash h$, then $P(s, C_m \& e) > P(h, C_m \& e)$ from 6 by P3A.

*9. $D(s, C_m \& e)$ from 7, 8, by R3.

*10. s is a member of I_e, from 9 and R3.

11. If s is not a member of I_e, then s is a member of I_e, from 3-10.

12. s is a member of I_e, from 11.

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Reviews

SHILS, Edward, Ed. (1968) *Criteria for Scientific Development: Public Policy and National Goals*. Cambridge, Mass.: M.I.T. Press. $8.95. Pp. xvi+207.

This selection of fifteen articles by ten authors is drawn from the 1962–7 issues of *Minerva* by the editor of that periodical. In spite of the wide range of concerns of the authors and their divergent views, the collage is almost held together by references in some of the later articles to earlier ones and particularly by the two articles by S. Toulmin which not only incisively analyse the views expressed in several of the other articles but also correlate these views with a wider literature.

The appearance of the book is timely, for we are at a critical point in the genesis of governmental science policies in the West. In fact, as judged by references to their *Minerva* articles in other books and essays, we must recognise the prescience of M. Polanyi, A. M. Weinberg, and Toulmin, among others of the ten authors, in helping lay the groundwork for the present lively dialogues flooding the pages of the scientific and popular press.

Essentially the articles fall into three classes: science policies for the sake (1) of science *per se*, (2) of society in developed countries, and (3) of society in underdeveloped countries. Four of the articles are of the last class and two articles by Polanyi comprise the first class. The feature which most disjoints the book is that the articles by Polanyi are prominently placed, being chronologically the first and last in the series, yet he wrestles in a completely different arena from the other contestants. The choice of a problem Polanyi leaves strictly to the scientist. Purely *laissez faire*, he holds that '. . . in the pursuit of excellence . . . [science] offers no part to the popular will and accepts instead a condition of society in which the public interest is known only fragmentarily . . .'. External control could be fatal: 'You can kill or mutilate the advance of science, you cannot shape it.' (Lest one critically think that mutilation is a shaping of sorts, we recall the comment of Edward McCrady relative to the negative results of Professor Weissmann's many-generations experiment on amputating the tails of new born rats: There's a divinity that shapes our ends, Rough-hew them how we will.) As regards the aspirations of society to guide the progress of science into socially beneficent channels, Polanyi sees this '. . . aim to be impossible and nonsensical'. The other nine authors address themselves to this quixotic quest. In fairness, it should be noted that Polanyi's 'Republic of Science' could be defined as that part of science that functions within his ideal boundaries and that the rest of the world, whether tinged by science or not, is simply foreign territory.

Weinberg articulates the assumptions that are implicitly or explicitly accepted by the other contributors: the development of science today is guided by the financial support available to scientists; there is a limit to the financial support which society can devote to science; therefore, society must choose what scientific efforts to support. Whereas Polanyi would have society support able scientists *qua* able scientists, leaving priority decisions to individuals, Weinberg argues that choices of fields and institutions to be supported are best made not on the basis of internal criteria, 'How well is the science done?', but of external criteria, generated outside the scientific field, focused on the question: 'Why pursue this particular science?' Listing three criteria, technological merit, scientific merit, and social merit, Weinberg illustrates the problem with five examples of high-cost programmes: molecular biology, high-energy physics, nuclear energy, manned-space exploration, and the behavioural sciences. He feels such incommensurable endeavours cannot be competently ranked in terms of support priority by the investigators themselves. He gives high marks in scientific merit to the field that "... contributes most heavily to and illuminates most brightly its neighbouring scientific disciplines." In a review of another book (*Science*, 163, 61, 1969), R. E. Marshak rejects this criterion as applied by Weinberg, who comes down in favour of the first and third of his five examples.

In a paragraph on the Chalk-and-Cheese Principle, Toulmin points out that at each stage in the decision process a choice should be made between commensurable alternatives. How to achieve this ideal in deciding which sciences to support is never made clear, much less the even harder decision of what portion of a nation's limited resources to devote to science at all. C. F. Carter and J. Maddox consider the choices that must be made relative to the distribution of support and the effect of this economic lever in guiding scientific development. Carter proposes that factors limiting economic growth be identified, for example, Britain's need to export, and that support be provided for science and technology in economically critical areas. Maddox finds that because society has come to regard science as an instrument for transforming its way of life, that these expectations of the public are most important in the public administration of science. Both Carter and Maddox take it as given that it is a governmental responsibility to guide science along potentially socially beneficial lines and that wise choices can be made. B. R. Williams, whose paper is on economic growth related to outlays for research, examines the production records of many nation and concludes that "... internationally there is no sign of a high correlation between rates of growth in output per head and the percentage of gross national product devoted to research and development". He even raises the spectre of excessive research and development hindering economic growth by employing scientists who could be more profitably used in production industries, particularly in underdeveloped countries. S. Rottenberg considers in economic terms the place of pure research: is it valuable for its potential effect on output, or for its aesthetic appeal to the investigator, or for the pleasure it gives its patrons, private or public?

Articles by S. Dedijer, M. J. Moravcsik, and A. Salam are concerned with science in underdeveloped countries. There is a striking difference in their concerns and those of the other authors. There is no question about how: it must be by government fiat. There is no question about how much: it must be as much as possible. Whereas the others are concerned with justifying their cases to the public, these latter writers assume that the critical decisions must be made by rulers, not by a poorly informed populace. With such a great need for wisely using the limited resources available, it is depressing to have catalogued the waste, misdirected zeal, lethargy, empire building, and chicanery which make it difficult for even devoted and able scientists to function in some countries. All three of these authors agree that an indigenous basic research effort is the *sine qua non* for the underdeveloped country that seeks to raise itself by its technical bootstraps.

Since the first of these articles was written science policies have been evolving rapidly. Canada, U.K., and France have new programmes for subsidising the development of civilian technology. In the U.S., consideration has been and is being given to the establishment of a science department at cabinet level. And the National Science Foundation is encouraging individual states in the U.S. to develop science policies for co-ordinating federal and local science programmes, for stimulating the economy, for increasing skills and abilities of workers, and for using technology to protect the environment. These articles from *Minerva* provide an interesting background for current developments.

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MISCHEL, Theodore, Ed. (1969) *Human Action, Conceptual and Empirical Issues.* London: Academic Press. 65*s*. Pp. xi+293

Inquiry is increasingly specialised. One bad result is that workers in one discipline find it hard to influence, or even communicate with, those in other fields, including adjoining ones. One remedy attempted is to set up a conference. This book collects papers on human action given at a small gathering of psychologists, on the one hand, and philosophers concerned with philosophical psychology, on the other. It is a deplorable advertisement for such willed co-operation. Real intellectual cross-fertilisation and bridge-building occur when a genius such as Chomsky proposes some system of hypotheses that provides a natural focus of interest for different disciplines. Then, and perhaps only then, things flow together in a way that seemed antecedently impossible. Nothing of the sort occurs in this book.

The philosophers at this conference (Mischel, Toulmin, Peters and Melden) all belong to that somewhat loose tradition of thought about philosophical psychology that springs from Wittgenstein and Oxford linguistic philosophy. I think they illustrate that, despite the great, indeed immeasurable, services of this tradition, it is now decadent, with little to offer but stale repetition.

The central assumption in all four papers is that the explanations of human action given in ordinary life in terms of reasons, intentions, wants, emotions and so on, are not *causal* explanations. They do not bother to argue against the many attempts made by other philosophers in recent years to meet the Wittgensteinian arguments and go back to the classical view that such explanations *are* a species of causal explanation.

Toulmin, for instance, argues that to explain the significance of particular pieces of behaviour (their meaning or the intention behind them) is to relate them to:

> the wider constellations of behaviour, linguistic and non-linguistic alike, within which they are located (p. 93).

But suppose, for example, that we explain A's doing X on a particular occasion where circumstances were Y by pointing out that it is a rule of his to do X in such circumstances. We are certainly not simply pointing to the fact that *on other occasions* when circumstances are Y then A does X. Such a "wider constellation of behaviour" could, conceivably, be a mere coincidence. It is further necessary that his behaviour in each case *spring from*, that is, be caused by, one and the same thing within him. As Peters seems to recognise briefly (p. 140), rules of conduct, if they are to function as explanatory principles of particular pieces of behaviour, must be *internalised* in the individual that acts. It is this internalised principle that is the cause of action (in conjunction, of course, with many other causal factors in the situation) on suitable occasions. What such an internalised rule could be, and how it could act as a cause, are problems that require a good deal of thought. But Toulmin's preferred style of analysis of explanations of behaviour is demonstrably incomplete, and cannot be made complete, I think, without the introduction of such causes in the individual.

I quote, if only to illustrate the depths to which this tradition can sink in its polemic against mental causes, the following passage from Mischel:

> . . . if a cognitive process is something different from the subject's verbal report, then what is it? Are we again being asked to identify thought with mental imagery? (p. 36).

Of the philosophers' papers I think Peters' is the best. It contains some interesting remarks about the emotions.

Among the psychologists (Campbell, Atkinson, Arnold and Madison) Atkinson's paper is an example of the sort of stuff that has convinced opponents of psychology that the discipline is worthless. Windy high-level generalisations are given specious precision by a series of pseudo-mathematical formulae. They yield a conclusion like this:

> . . . other things equal, the person described as strong in achievement motive should be more willing to initiate achievement-oriented activities when presented with challenging opportunities in his environment and should be more persistent in them when confronted with opportunities to engage in other kinds of activity than the person described as weak in achievement motive (p. 130).

Molière, thou should'st be living at this hour. Psychology could do with your help.

Peter Madison, however, contributes a paper that is modest, sensible and thoughtful. He accepts from the philosophers at the conference the view that:

> . . . distinctions made in our everyday language are far richer and more complex than those made in psychological theory today, and that, consequently, common sense is not to be dismissed as lightly as psychology tends to do. In fact, it should be regarded as our principal source of theoretical knowledge about man, to which psychology has only added in modest ways as yet (p. 253).

The establishing of this point, in the course of innumerable detailed investigations, I take to be one of the great triumphs of this philosophical tradition. (Even if the tradition now stands in the way of our further understanding of the very distinctions it called attention to.) But Madison is not led to turn his back on the simplified situations and the deliberately narrow range of factors studied by modern experimental psychology. What the psychologist should do rather, he thinks, is to move back and forward between such laboratory studies and the study of complex human behaviour in natural settings. This, he suggests, will be the best remedy available for overcoming the characteristic disadvantages, equal and opposite, that afflict both methods of work, and will enable them to be brought together slowly.

There is also an interesting paper by Donald Campbell. He is concerned, among other things, to reject any "incorrigible evidential base". I think that his enthusiasm for this laudable objective may get the better of him when he asserts that "all knowledge claims go beyond their evidence" (p. 42). Will not the "evidence" itself have to be a knowledge claim? And will this not produce a vicious regress at the end of which all evidence will have vanished? Campbell also develops an interesting model of consciousness as a "wire-tap" on our own mental processes, and offers a fascinating, if hard to make precise, suggestion to explain one obtrusive phenomenological feature of consciousness: the dominance of perceptual elements and the relative absence of motor awareness. In the passage between stimulus and response, he suggests, the "wire-tap" is located after sensory stimulus and thought have done their work but before the response is initiated.

The worst news in this book is to be found in its last sentence. It tells us that another conference "focussed on psychological and epistemological issues in the development of concepts" is being planned.

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PUCCETTI, Roland (1968) *Persons: A Study of Possible Moral Agents in the Universe.* London: Macmillan. 50*s*. Pp. ix+145

Puccetti asks whether the class of *persons* might include non-human persons such as an immaterial God, sophisticated machines, artefactual organisms, and natural extra-terrestrial or extra-galactic organisms. He reviews the empirical evidence for extra-terrestrial intelligence, and asks whether we should expect such alien persons to be like us biologically, psychologically or socially. Finally, he explores some of the wider (moral and religious) implications of belief in such beings. The universal scope of this short book results in a less detailed discussion of particular points than one might wish. The philosophical arguments are sketchy, for instance those dealing with Strawson's account of persons and with the ascription of psychological predicates to machines. But the empirical discussion is interesting, and many of the questions raised have a great intrinsic fascination.

The focus of Puccetti's rebuttal of Strawson is his claim that a person is essentially a moral agent, which requires both the ability to master a symbolic conceptual system, and the capacity to experience certain feelings, sensations and emotions. Puccetti doubts that God can be a true person, not on the grounds of His incorporeality *simpliciter*, but because as an immaterial being He is said to be incapable of such experiences and so cannot truly be our moral Judge. Puccetti here assumes that an essential feature of a moral judge is the ability to understand or empathise with the motives and felt impulses of the agent being judged. The focus of his refusal to ascribe psychological predicates to machines is the material difference between the components of machines and men. Because of the biological functions of pain and emotions, we have good reason for ascribing comparable experiences to any naturally evolved soft organisms living in a world of hard objects—and also to artefactual organisms, should these ever be produced in the biological laboratory. But we have no reason to expect that inorganic (rigid and metallic) machines, however sophisticated, should experience such feelings; therefore they are not possible persons, and could not be moral objects for us.

Puccetti produces interesting biological arguments against the sort of "biological liberalism" which allows that extra-terrestrial organisms might be *very* unlike us: for instance, that they might be spheres which manipulate objects internally much as we manipulate things with our tongues. Biological features such as cephalisation, bilateral symmetry, and the possession of two pairs of jointed limbs may be universal to highly complex organisms. And he argues (in line with Hart's account of the minimal content of natural law) that any group of tool-using, predatory, symbolising and communicating organisms must have a moral and legal code basically resembling ours. This means that we might form some sort of moral community with extra-terrestrial organisms of this type. The belief in extra-galactic intelligence he holds to be scientifically based but unconfirmable, due to the vastness of the distances involved. Even electromagnetic signals, travelling at the speed of light, could not in practice inform us of such intelligence. And the practical difficulties of confirming the belief in *intra*-galactic intelligences are very severe, as Puccetti shows in a fascinating discussion of the evidence. But despite the difficulties of confirmation, Puccetti regards belief in non-human intelligences as supported by science and as compatible with philosophy.

Whether it is compatible with the so-called "universal" religions is, he says, another question. He claims that religious "truth" can only be defined in terms of the acceptance of the authority of a particular living religious tradition. Hence there are grave difficulties in the notion that Christianity, Judaism or Islam might be truly *universal* creeds applying equally to all moral agents in the universe. Our theistic traditions all lay great stress on a particular *human* being, whether he is regarded as the messenger or as the incarnation of God. We could hardly expect the denizens of Alpha Centauri to worship a human Christ or venerate a human Buddha. Christian orthodoxy cannot admit contemporaneous incarnations in different parts of the universe, because of the creed's insistence that Christ (in human form) is *the same person* as God. Whatever difficulties there may be in accepting this dogmatic claim, Puccetti considers it clear that the bodily Christ *cannot* be the same person as any other contemporaneous body (whether human or not). He concludes that only by reverting to the docetic heresy (that God took on the *appearance* of the man, Jesus) can any sense be made of the notion of contemporaneous incarnations—and the theological force of "incarnation" is thereby weakened. Puccetti here probably assumes too quickly that ingenious theologians would not be able to produce a recognisably "Christian" doctrine which could allow that all moral beings whatever form part of one moral community under God: non-fallen persons would not require a Saviour.

Other topics (such as the personal afterlife) are briefly raised by Puccetti. But the main distinction of the book lies in its discussions of the empirical evidence concerning alien intelligences and the ways in which we may reasonably hope to be informed of them.

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DRETSKE, Fred I. (1969) *Seeing and Knowing.* London: Routledge and Kegan Paul. 35*s.* Pp. ix+261.

Professor Dretske's book is about "what we see and how we see in so far as the answers to these questions help us to get clear about what we know and how we know it" (p. 2). The bulk of the work is general philosophy of visual perception, but the final chapter is "an attempt to show how a deeper appreciation of observation itself can provide us with a new basis from which to attack some of the traditional issues in the philosophy of science" (p. 202). I shall sketch the main lines of his general account with an eye towards the final chapter, which is the part of the book likely to be of most interest to readers of this journal.

The author's basic thesis is that, despite the immense variety of ways of describing our visual efforts and achievements, "there are just two fundamental themes which can be traced throughout the spectrum of differences, two fundamentally different, though not unrelated, ways of seeing things which can be found, either singly or jointly, in every distinctively visual situation" (p. 1).

The first of these is "non-epistemic seeing" (ch. II). 'S sees D non-epistemically'—for short, 'S sees$_n$ D'—only if the truth of the latter does not entail the truth of 'S believes P' where 'P' is some proposition (assertion of belief). Positively speaking, 'S sees$_n$ D' is equated with 'D is visually differentiated from its immediate environment by S'. Seeing$_n$ is "a primitive visual ability" (p. 4); what we see$_n$ "is a function solely of what there is to see and what, given our visual apparatus and the conditions in which we employ it, we are capable of visually differentiating" (p. 76).

The second of the two schemata is "epistemic seeing" (chs. III-V), i.e. the acquisition of beliefs by visual means. Seeing a table is an instance of seeing$_n$; seeing that something is a table is an instance of epistemic seeing. It is of two sorts, "primary" and "secondary", the first being the fundamental one. An example would be my seeing that the table on which I am writing is brown by looking at it. 'S sees that b is P in a primary epistemic way' if and only if (i) b is P, (ii) S sees$_n$ b, (iii) the conditions under which S sees$_n$ b are such that b would not look, L, the way it now looks to S, unless it was P and (iv) S, believing the conditions are as described in (iii), takes b to be P. An example of "secondary epistemic seeing" would be my seeing that my cigarette lighter is low on fuel by looking at the flame. '*S* sees that *b* is *P* in a secondary epistemic way' if and only if (i) *b* is *P*, (ii) *S* sees *c* ($c \neq b$) and sees (primarily) that *c* is *Q*, (iii) conditions are such that *c* would not be *Q* unless *b* were *P*, and (iv) *S*, believing the conditions are as described in (iii), takes *b* to be *P*.

Professor Dretske insists that, despite the difference between them, both forms of epistemic seeing are purely visual achievements; that is, both are direct, non-mediated ways of acquiring knowledge by using our eyes. Certainly the second involves conceptual elements which the first does not; but to draw a radical distinction between them on this ground would be to confuse what occurs with the conditions under which it occurs. In both modes of epistemic seeing a belief is generated or verified as a direct result of a visual experience occurring under certain conditions: this is what happens, and is the basic point. "The chief difference between primary and secondary seeing is that, under critical pressure, we can more easily be brought to qualify, hedge, or even retract our latter perceptual claims; and this is so because the achievement which we report depends, for its successful realisation, on a more elaborate network of regularities" (p. 161).

The author argues, against the sceptic, that *S* can know, and indeed know for sure, that *b* is *P* when he sees *b* under conditions (i)-(iv), even if *S* does not or cannot meet the sceptic's demands for a full justification of the background beliefs involved in (iv). "The only thing that *S*'s having a justification for his background belief lends to this situation is an *assurance* to *S* that the way *b* looks *is* a conclusive reason in the above sense. But *S* does not *need* this assurance since he already . . . believes that the way *b* looks *is* a conclusive reason in the above sense. . . . In relation to background beliefs, the relevance of justification is psychological, not logical. Most men require some degree of justification before they believe something, but what makes a background belief effective in allowing us to see what is the case is not its justification, or how (antecedently) justified we are in having it, but the fact that it is a *belief* and the fact that it is *true*" (pp. 134 f.).

The above is the indispensable foundation for the chapter on observation and scientific practice (ch. V). To begin with, 'X is (visually) observable' if and only if X can be seen$_n$ or can be seen epistemically to obtain; and 'Y is an observation (for *S*)' if and only if *Y* is something which *S* sees to be the case and is relevant or thought to be relevant to his inquiry. Observations are then distinguished, in an obvious way, into primary and secondary kinds. These are similar in being direct acquisitions of beliefs by visual means, and differ in the *sort* of background beliefs involved. A consequence of this position is the rejection of the "meta-linguistic" treatment of observation (e.g. 'Is this term an observation term or not?'). "One must ask about specific information whether it was seen to be the case or not, and it if was, whether this was in a primary or secondary fashion. The answer to these questions will depend on who acquired the information, what he was assuming when he acquired it, what he saw (non-epistemically) in the process, and the conditions under which he saw it" (p. 216, and cf. also pp. 99 f., 190 ff., esp. pp. 200 f.).

Professor Dretske goes on to explain the sense in which, on the basis of his theory, it is possible to "see the general in the particular". This is by way of the notion of a "systematically loaded concept" (e.g. electrical resistance R), which is one where (i) there is a lawful or theoretical relation between two or more of the magnitudes in terms of which the concept is defined (in the example given, Ohm's Law connecting applied electromotive force E and current intensity I), and where (ii) the application of the concept is restricted to those cases in which this relation holds. Then "one's observations can assume an essentially generalised form . . . when the particular state of affairs with which one is confronted is, as a matter of fact, a manifestation (in these conditions) of a uniformity or regularity of some sort, or when it is connected with the application of a concept which itself presupposes such a uniformity" (p. 227). (E.g. '$R = 9$ ohms at 12 noon' entails 'at noon *any value of* E would have given $I = E/9$'.) He notes, with regard to the traditional problems of induction, that observations can assume a generalised form only when either the knowledge presupposed in the observational context ('this circuit has a resistance') is already general or "the observing takes place in a context of background beliefs which themselves mediate between the particular and the general" (p. 232). But "the *justification* of one's background beliefs is not necessary to their effective operation in seeing (epistemically) what is the case. All that is required of these beliefs is that they be *true* and *assumed* to be true. . . . So . . . the so-called "inductive gap" *can* be bridged by observation." But this procedure does not significantly by-pass the standard inductive techniques because "(1) we are generally unwilling (or unable) to assume something which we have no reason to think true, and (2) even if we were willing and able, the probability would be overwhelmingly great that what we assumed would be false" (p. 233).

The author next takes up the argument that a quantity (e.g. height), in so far as it is fully metricised, is unobservable, because "no *one* of its possible values can be observed to the discrimination of that infinite number of other possible values lying within the "reasonable margin of error" " (p. 235). His reply is that there is no *general* answer to the question whether the exact value of such and such a quantity is observable, for the answer varies according to the particular situation. What in particular is being measured determines the level of meaningful precision in that instance (e.g. tables cannot be measured in micro-inches because the limits of a table are just not specified with sufficient exactness); and the measuring device available will determine whether we can distinguish between the various values at this level of precision.

The final section of the book is an examination of the view that a scientific theory about the constitution of what we see reveals what it is that we have (really) been seeing all along. His conclusion is that an adequate theory does do this, though he thinks that this does not follow from a simple substitution-argument (e.g. 'We see tables, tables are identical with ordered collections of molecules, and therefore we see ordered collections of molecules'). Rather it follows from "the fact that the sense of the verb 'to be' in the equation of a table with a system of molecules is a sense of the verb which allows us to say that when we see the table, the system of scientific constituents with which it is to be identified must look some way to us" (p. 253). He concludes with a discussion of the view that the system of, e.g., molecules, can be significantly said to have all the properties, intrinsic and relational, which can be significantly ascribed to, e.g., the table composed of the molecules.

This last portion was the only part of the book which I found (and continue to find) difficult to understand. The overall clarity may well excuse some prolixity and repetitiousness in the presentation.

As regards the doctrines presented, a few indications of where some queries might begin must suffice. The notion of non-epistemic seeing seems to allow for the seeing$_n$ of private objects of various sorts (e.g. hallucinations) (pp. 44 ff., esp. top of p. 46), which are well known trouble makers in the philosophy of perception. Again, it is not clear that the sceptic will be much impressed by the arguments against him: he is after all arguing in general not that we never satisfy the conditions for knowing something but that, even if we sometimes do, we cannot distinguish these cases from those in which we do not. If this is a tenable objection then it is also dubious what new light is cast on the problem of the "inductive gap" by the considerations in the last chapter.

Professor Dretske's book will repay reading by anyone interested in the field which it treats.

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CLAVELIN, Maurice (1968) *La Philosophie Naturelle de Galilée.* Paris: Armand Colin. N.p. Pp. 504

This book attempts to answer the question: with the aid of what conceptual tools and through the erection of what theoretical framework did Galileo give birth to the modern science of mechanics? Since the demise of the myth of Galileo as a great experimentalist, it has been tempting to describe him as a hypothetico-deductivist, who, after working out the solution to the problem of falling bodies and projectile motion, proceeded to apply his new science to the astronomical questions that bedevilled his contemporaries. At first sight, this sounds plausible enough, since Galileo's major contribution to the debate on Copernicanism was to reveal the fallacy of the traditional common-sense arguments against the motion of the earth by restating them in mechanical terms. On closer examination, however, this hypothesis is not borne out. Galileo discussed the question of motion within the framework of two distinct and particular sets of problems: the justification of the heliocentric theory, which he dealt with mainly in the *Dialogo* of 1632, and the acceleration of falling bodies and the path of projectiles, which he treated extensively in the *Discorsi* of 1638. The two sets of problems are not completely independent—Galileo did not work in two separate mental workshops—but they cannot be said to form a unified whole. For instance, inertia is circular in the *Dialogo* whereas it becomes linear in the *Discorsi*.

The author argues, quite persuasively, that the more adequate discussion in the *Discorsi* resulted from Galileo's desire to account for motion geometrically, and he sees the reintroduction of the Archimedean ideal of geometrical explanation in the sixteenth century as the promixate source of his methodology.

He also stresses the importance of Galileo's analysis of motion along an inclined plane in terms of *momento*. This concept contained in an undifferentiated form not only the notions later to be distinguished as static moment and momentum, but the useful if ambiguous "moment of descent" whereby the retarding effect of the inclined plane was acknowledged to be the same everywhere. This implicitly separated the effect of weight from that of gravitation, and paved the way for an understanding of motion as a state rather than a process.

In spite of its obvious merits, this book suffers from certain shortcomings. First, it is too long and too discursive. The introductory chapters on Aristotelian physics and fourteenth-century science are not original, and they have little relevance to Galileo's own contribution to mechanics. Secondly, the author seems to accept at face value Koyré's thesis that Galileo's experimental claims are spurious. This is particularly unfortunate in view of the fact that on Clavelin's own account Galileo was not a thorough-going rationalist. Thirdly, by sharpening the concept of *momento*, the author has made Galileo appear conceptually more advanced than he may have been. But these are minor details, and when all is said, the book remains a useful introduction to Galileo's new science of motion.

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Recent Publications

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Bertrand Russell as Philosopher of Science

by R. B. BRAITHWAITE

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THE EDITOR asked me, immediately after Russell's death on 2 February 1970, to write something for this *Journal* about his contribution to philosophy of science. There is a difficulty in doing so, owing to the vague boundaries of the term 'philosophy of science'. If this is taken to include the foundations of mathematics and general questions about the basis of empirical knowledge, there is no question as to the importance and influence of his writings.

The greatest and most original of Russell's contributions to thought is his paper 'Mathematical Logic as based on the Theory of Types' (*American Journal of Mathematics*, 1908), expanded in 1910 in the first volume of *Principia Mathematica.* It is this work which ensures Russell's immortality. Though the particular Theory of Types invented by Russell has been improved, and alternatives proposed, by later mathematical logicians, the notion introduced by Russell of a hierarchy of language-systems, each using symbols which are meaningless outside the language-system in which they are properly employed, was a turning-point in logic (and not only in the logic of mathematics). For, together with Russell's treatment of "definite descriptions" as "incomplete symbols" which cannot be given a meaning in isolation from the sentences in which they occur ('On Denoting', *Mind*, 1905), his work opened the way to the realisation that thought cannot be discussed independently of the language-system in which the thought is expressed; and this I take to be the essence of the "revolution in philosophy" associated with the name of Wittgenstein. If Wittgenstein was the Robespierre of this revolution, it was Russell who set fire to the Bastille.

But this is hindsight. In the twenties and thirties Russell's dominating position in Anglo-American philosophy was due to the leading part his writings from 1910 onwards played in the revival of the philosophy of empiricism. Like his predecessors Locke, Berkeley, Hume, Mill in the British empiricist tradition, Russell wished to base all empirical knowledge upon immediate experience, and he believed that the way to do this was by a method of "logical construction" which had worked so well in mathematics: 'The supreme maxim of scientific philosophising is this: *Wherever possible, logical constructions are to be substituted for inferred entities*' (1914). Russell aimed at analysing propositions about material objects as propositions about "sense-data"; and in *The Analysis of Mind* (1921) he aimed at doing the same for propositions about mental entities. (This book is also notable for going along with behaviourism as far as possible.) Russell gave special attention to the analysis of "physical" space and time into "perceptual" space and time. In his later writings he regarded the general problem as that of establishing an identity of relational structure between the realm of perception and that of the physical world, without always realising that to say that two realms have the same structure is to say very little unless the ordering relations in both realms are given.

This work of Russell's is "scientific philosophising" in the field of general epistemology. In many of his popular and philosophical books Russell included expositions of relativity theory and of parts of quantum physics (his *ABC of Relativity* (1925) did great service in emphasising that the "observer" of relativity-theorists could perfectly well be a camera or other recording device) but he rarely wrote on the philosophical problems that arise within a science itself. In his last major work, *Human Knowledge: its Scope and Limits* (1948), he discussed the sort of probability that occurs in statistical laws (which he distinguished from probability as "credibility"), and came down in favour of a "finite frequency" interpretation rather than a "limiting-frequency" interpretation. But, disappointingly, he did not work out the consequences of his finite theory. Although what is now known as the hypothetico-deductive (HD) view of the meaning of scientific hypotheses containing theoretical terms stems directly from Russell's attempts at reduction by the method of logical construction, he does not appear to have realised the force of Ramsey's demonstration (published in 1931) that, were such a reduction possible, a hypothesis would lose its ability to function in the future in a new system of hypotheses. Russell saw that the function of certain logical terms was to license deductions from propositions containing them; but he did not pass from this to the HD view that this is also the way in which theoretical terms in a scientific system function. It would be possible to gloss Russell's statement, that all we know about the world of physics is that it has a common structure with the world of sense, into a statement of the HD view. But Russell did not make this gloss. In *Human Knowledge* he explicitly says of two complexes having the same structure: 'Hence arises the possibility of a dictionary, by means of which statements about the one complex can be *translated* into statements about the other' (my italics).

However there is one specific contribution which Russell made to the philosophy of physics. Like Whitehead, Russell preferred to take as the basic elements for physics events rather than things. This for him required that he should try to "logically construct" space and time out of relations between events. Whitehead had done this by a "Chinese-box" construction which presupposed that there should be no minimum or maximum to the size of events. Russell found this unacceptable; and worked out an alternative construction in terms of a relation of "overlapping" between events which was compatible with there being both minimum and maximum events. His construction for instants of time was given in *Our Knowledge of the External World* (1914), and in a paper in the *Proceedings of the Cambridge Philosophical Society* (1936) he proved that the existence of such instants required "hypotheses which there is no reason to suppose true—a fact which may be not without importance in physics". In *The Analysis of Matter* (1927) he attacked the problem of space, and of space-time, employing topological techniques. The basic relation between events which he used in his construction was a pentadic relation of "co-punctuality" holding between five events when there is a region common to all of them. If a "field" type of theory comes to triumph over a "particle" type of theory in fundamental physics, so that physicists (consistently) adopt an event ontology, Russell's work of 1927 and 1936 on what is required of events in order that they should generate a continuous space-time will become essential reading for philosophers of physics, especially to those who would wish to give more "reality" to the field and its waves than a purely hypothetico-deductive account would provide.

Causality was discussed at various times by Russell, usually in a negative and critical manner, since Russell throughout his life thought of causation in a Humean way as a matter of regular sequence or concomitance. However in *Human Knowledge* he makes great use of the notion of "separable causal lines" in the postulates which he puts forward to "validate scientific method"; and a careless reader may easily believe that Russell has in mind some more-than-Humean conception of causality. This is not the case: Russell nowhere propounds a distinct notion of "causal implication" or constructs a modal logic for it (as several philosophers have tried to do). Indeed for his purpose in providing some sort of justification for induction Russell's emphasis on causal lines is circular, for he *defines* a causal line as "a temporal series of events so related that, given some of them, something can be inferred about the others whatever may be happening elsewhere". (The inference, of course, only yields the sort of probability that Russell called 'credibility'.) So Russell's very interesting discussion of the place of separable causal lines in our thinking about nature must be taken as an analysis of scientific method and not as its justification.

I must not conclude this tribute on a negative note. Though Russell's greatest and most original work was in fields wider than philosophy *of* *science*, it is not much of an exaggeration to say that, without Russell's "scientific philosophising", there would have been no subject philosophy of science as we understand it today. The fact that in the 1910s and 1920s the co-author of *Principia Mathematica* was preaching to philosophers that logic was "the essence of philosophy" (and in books many of which are models of lucidity and grace) as well as suggesting new and exciting ways in which "the logical-analytic method" could be used, made philosophers of science out of young thinkers who might otherwise have been the woolliest of metaphysicians. Russell was then almost the only living British philosopher to be known outside English-speaking countries: as is well known, the Vienna Circle owed much to his influence. When Russell arrived at the *Congrès International de Philosophie Scientifique* held in Paris in 1935, he was universally acknowledged as the *doyen*—almost as the *guru* (though I, for one, felt the shade of Mach to be present as co-*guru*). The state of philosophy of science would be very different now from what it is had not the physicist Ernst Mach and the mathematical logician Bertrand Russell both taken to "scientific philosophising".

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Curie's Principle

by A. F. CHALMERS

INTRODUCTION

In his paper 'On Symmetry in Physical Phenomena' Pierre Curie (1894) proposed what seems to be an intuitively plausible relationship between the symmetry of an effect and its cause, namely that "When certain causes produce certain effects, the elements of symmetry of the causes must be found in the effects produced" (p. 401). A striking feature of the principle so stated is its generality. It would appear to place restrictions on the outcome of any physical situation. But if the principle is valid and can be made sufficiently precise for its apparent power to be realised, it is puzzling to note that it is rarely referred to or explicitly utilised in the physical sciences. Reasoning based on notions of symmetry has been evident in science since antiquity[1] but the arguments employed are usually vague or implicit.[2] Even if the sole function of the principle is to render such reasoning more precise and explicit an exposition of it is desirable in so far as it clarifies the status of and the assumptions involved in the arguments. One explanation of the lack of reference to the principle would be that it is implied by some other principle in common use. We are led to ask, then, if Curie's principle can be derived from some more general assertions. We shall show that the principle can be derived from the invariance properties of physical laws but that this fact by no means renders the principle redundant.

Our remarks so far have been based on the assumptions that the principle is valid and can be formulated with sufficient precision for it to be of use in the physical sciences. If either of these assumptions were to prove false then we would, of course, have a very definite answer to our puzzle over the lack of attention paid to the principle. Freudenthal (1968, p. 420) has suggested that the principle acquires its generality by being necessarily vague. In this respect he attributes to Curie's principle a status similar to that of Leibniz's principle of sufficient reason.[3] That such a claim is

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[1] For example, according to Hippolytos, Anaximander argued that the earth "stays where it is because of its equal distance from everything". Burnett (1930), p. 64.

[2] This is not true in crystallography. Symmetry considerations were introduced in a formal way by crystallographers in the nineteenth century. Franz Neumann developed the idea that no crystal can possess a property that is more asymmetric than its structure (see Voigt (1910), p. 3 and p. 20). Curie's principle is an extension of it to physical phenomena in general, although there is no reference to Neumann in Curie's paper.

[3] One statement of this principle reads "no fact can be real or existing and no proposition

unjustified will be clear from the discussion in this, and indeed in Curie's, paper. At first sight at least, many physical situations, such as the magnetic field due to a linear current, or α-particle emission by a spherically symmetric nucleus, appear to refute the principle, so that doubts about its validity seem justified. If it is argued that a closer empirical investigation of the phenomena involved in these situations will lead to symmetry assignments consistent with the principle, then the suspicion arises that there is a circularity involved in the application of the principle. It would seem that an apparent refutation of the principle can always be discredited by postulating some, as yet undiscovered, asymmetry in the cause in the refuting instance. We shall see that, in spite of this irrefutability, there exists a wide class of circumstances in which the principle has predictive power, and just because of it, the principle can be a powerful heuristic guide to the formation of new theories.

One aim of this paper is to present as precise and as general a formulation of Curie's principle as possible. With such a formulation at our disposal it is possible to answer some of the questions relating to the status and scope of the principle. Having introduced the terminology necessary for our symmetry considerations in 1, we explicate the principle in 2. The way in which the principle can be applied to determine the symmetries of physical quantities and systems is indicated. Further light is thrown on the fundamental status of the principle by the investigation, in 3, of its relation to the invariance properties of physical laws. The principle follows from the latter for deterministic laws. The significance of these basic considerations is illustrated by applying them, in 4, to some physical situations, particularly those that appear to refute the principle. One remaining problem, the significance of the asymmetry of the principle with respect to the interchange of the terms 'cause' and 'effect', is discussed in 5. The status and uses of the principle are summarised in 6, the concluding section.

1 SYMMETRY TRANSFORMATIONS

In order to carry out the programme outlined in the Introduction it is necessary to introduce some formal definition of symmetry. We do this in as simple a fashion as possible, introducing technical detail only when it is essential for an argument or for the illustration of some example.

If a physical system is described by some specification of its properties (such as temperature, electric field strength, the presence or absence of

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can be true unless there is a sufficient reason, why it should be thus and not otherwise" (see Morris (1934), p. 9).

certain types of molecule) then we define a *symmetry transformation* or *symmetry operation* for the system as an operation performed on it which leaves the specification unchanged. The description of a system will always be some abstraction which will depend on the conceptual framework in which, or the theoretical standpoint from which, it is viewed, so that, in a sense, a symmetry transformation is a property of the description rather than of the physical system itself. The description of a cube with faces of different colours may have cubic symmetry when the cube participates in some mechanical experiment in which the colour of the faces is irrelevant, but some quite different symmetry in an optical experiment involving reflection from its faces. If in some physical situation only the relative shapes and positions of the component parts of a system are considered relevant, then translation of the system or a uniform change in scale may be a symmetry transformation of the system. Symmetry transformations can equivalently, and often more conveniently, be defined in terms of a transformation of the co-ordinate system with respect to which a system is described rather than of the system itself.

All the essential points that we wish to make concerning Curie's principle can be made with reference to those *orthogonal* transformations which do not involve a translation. When treated as transformations of the co-ordinate system, the orthogonal transformations are those that transform one Cartesian co-ordinate system into another without change of scale. When treated as transformations of a system relative to a fixed co-ordinate system, they are those that leave $\{(x_1-x_2)^2+(y_1-y_2)^2+(z_1-z_2)^2\}$ invariant, where $(x_1y_1z_1)$ and $(x_2y_2z_2)$ are the co-ordinates of any two points in the system referred to the same co-ordinate system. The shape and size of a system are preserved under these transformations. The members of the complete set of orthogonal transformations form a group O. The members of the set of orthogonal transformations which are symmetry transformations for some physical system will form a group which is a sub-group of O. Any two transformations in such a set, carried out successively, will give the same result as another single transformation in the set. By omitting reference to translations from our discussion we restrict ourselves to point groups. The transformations which are elements of these point groups are equivalent, either to a rotation, in which case they are *proper* orthogonal transformations, or to a reflection, possibly accompanied by a rotation, in which case they are *improper* orthogonal transformations. Inversion through a point is an improper transformation which can be achieved by a 180° rotation about an axis followed by a reflection by a plane perpendicular to this axis. The planes, axes and points with reference to which the transformations can be specified are the *elements of symmetry* that Curie refers to in his statement of his principle, and are not to be confused with the elements of the point groups which are the transformations themselves. Four groups that are referred to repeatedly in the ensuing discussion have as elements the symmetry transformations of systems possessing an isotropic axis, that is an axis about which any rotation is a symmetry transformation. The group whose elements are the symmetry transformations that can be performed on a cylinder at rest will be denoted by '(*a*)'. The members of the sets of symmetry operations that can be performed on a system of two co-axial cylinders rotating in opposite senses will be denoted by '(*b*)', those on an arrow by '(*c*)', and those on a cylinder rotating about its axis by '(*d*)'. These all form groups which are sub-groups of (*a*).

2 THE SYMMETRY OF PHYSICAL QUANTITIES

The symmetry of purely geometric quantities with respect to spatial transformations follows directly from their definitions, which fix their relationship to the co-ordinate system to be transformed or with respect to which they are to be transformed. Indeed, this has already been assumed in 1 where symmetry groups were exemplified by the transformation properties of geometric figures. The introduction of time is straightforward; for example, it follows from the definitions of velocity and acceleration that each of these quantities has symmetry (*c*). It also follows from these definitions that acceleration is symmetric under time reversal while velocity is not, since it changes sign. It is worth stressing here that vectors are not spatially extended. The symmetry of velocity at some point is quite distinct from the symmetry of a velocity distribution. We introduce the term 'intrinsic symmetry' to describe the symmetry of a physical quantity defined at a point as distinct from the symmetry of a spatial distribution of this quantity.

The establishment of the symmetry of physical quantities or physical systems is not as straightforward as it is for geometric figures. The intrinsic symmetry of a physical quantity will depend on the nature of the theory in which the quantity occurs and the symmetry of a physical system will depend on the nature of the theory or conceptual framework in terms of which it is described. This point can be illustrated by considering the symmetry of a bar magnet in the form of a cylinder. A person having no knowledge or suspicion of the magnetic properties of the cylinder will ascribe to it a symmetry (*a*). An eighteenth-century physicist, however, might investigate the magnetic properties of the cylinder and decide, on the basis of its interaction with other magnets and the tendency of one of its ends to point North, that the ends have different properties, and accordingly characterise its symmetry by (*c*). A modern physicist, attributing the magnetic properties of the cylinder to what might be described classically as currents circulating within it, would be led to yet another conclusion and decide on (*d*) as the appropriate symmetry.

The following is a reconstruction of what I understand to be a routine procedure used by Curie to establish the symmetry of a physical phenomenon. The method assumes that each physical phenomenon is characterised by a unique symmetry and requires a knowledge of the symmetry of some physical quantities as a starting point. It also involves the use of Curie's principle but not in such a way that the principle loses all its content.

We first give a formal statement of Curie's principle in a form which is not restricted to orthogonal transformations of the co-ordinate system;

> When a cause is wholly responsible for a particular effect, all symmetry transformations of the cause must also be symmetry transformations of the effect.

The meaning of the terms 'cause' and 'effect' needs to be specified. In the examples considered by Curie the cause is invariably some part of the description of the system, at time t_0, say, from which the rest of the description at the same time t_0, regarded as the effect, follows. For example, a current distribution is regarded as the cause of the magnetic field that accompanies it. We wish to extend the notion to include those situations where the state of a system, at some time t_1, is regarded as the cause of that state of the system which necessarily follows it at some later time t_2. Thus the positions and velocities of a system of Newtonian particles can be considered the cause of their positions and velocities at some later time. We attempt to include both types of situation in one definition as follows:

> A physical situation at time t_1, described by $A(t_1)$, is said to be the cause of a second situation at time t_2 ($t_2 \geqslant t_1$), described by $B(t_2)$, and the latter is said to be the effect of the former, if $A(t_1)$ is sufficient to ensure $B(t_2)$.

There are, of course, difficulties associated with the demand for sufficiency. Some rather vague condition such as "these are the only relevant factors that obtain" must always be assumed to be included in the cause. For example, the positions and momenta of a system of Newtonian particles at some time will only be a sufficient cause of their positions and momenta at some later time if all perturbing influences are excluded.

Let us now spell out the procedure for determining the symmetry of some physical phenomenon *P*. It requires the application of Curie's principle to two experimental situations in which *P* is involved.

We first consider a physical situation in which the symmetry of all the phenomena occurring in the cause and effect are known with the exception of *P* which figures in the *effect* only. Application of Curie's principle to this situation will yield a *lower* limit to the symmetry of *P*.

We next consider a second physical situation in which the symmetry of all the phenomena occurring in the cause and effect are known with the exception of *P* which this time figures only in the *cause*. Application of the principle to this situation will yield an *upper* limit to the symmetry of *P*.

The symmetry of *P* can be uniquely determined if we can choose situations for which these two limits coincide.

It is clear that the method for the determination of symmetries which we have outlined is only applicable if the symmetry of a number of physical phenomena is regarded as known. We have already indicated how the symmetry of geometric figures and quantities such as velocity and acceleration can be deduced in a straightforward way. To proceed further it is necessary to adopt some empirically based physical theory. The symmetry of quantities appearing in classical mechanics can be established since mass is a scalar quantity in this theory. A sphere of uniform density will have the same symmetry as a geometric sphere, linear momentum will have symmetry (*c*) and angular momentum symmetry (*d*).[1] For the purpose of the immediately ensuing discussion at least it will be further assumed that a body uniformly charged with electricity has the same symmetry as the body when uncharged. This is in keeping with the modern conception of charge, treated as a scalar quantity, and is also in keeping with many of the nineteenth-century theories of the nature of electricity, such as the electrical fluid theories and some of the aether theories.

With these basic symmetries at our disposal it is possible for us to illustrate the method described above by using it to determine the intrinsic symmetries of the quantities appearing in electromagnetic field theory. We first determine the intrinsic symmetry of electric field. The argument differs little from that appearing in Curie's paper.

For stage one of our argument we consider the electric field at the centre of a parallel plate condenser made up of two oppositely charged discs. The cause of the field has symmetry (*c*), the line through the centres of the discs being the isotropic axis. We conclude from Curie's principle that the field at the centre of the condenser must have a symmetry which is at least (*c*). The intrinsic symmetry of the electric field, then, is at *least* (*c*).

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[1] The representation by an arrow of vectors with symmetry (*d*), such as angular momentum, is misleading from the symmetry point of view, as Curie points out, since an arrow has symmetry (*c*).

In stage two of the argument we consider a point charge placed at some point, *p*, in space where there is an electric field. The charge will experience a force which we take as the effect and which has symmetry (*c*). In the spirit of field theory we consider the electric field at *p* to be the sufficient cause of the force on the sphere and pay no attention to the details of the system that gives rise to the field. Application of Curie's principle leads us to the conclusion that the symmetry of the field at *p* is *at most* (*c*).

From those two simple experimental situations we thus establish that the intrinsic symmetry of an electric field is precisely (*c*).

The intrinsic symmetry of electric current is also (*c*). This can be concluded, for instance, by considering one situation in which an electric field is the cause of a current and another where electric current is the cause of chemical decomposition in electrolysis.

We can again closely follow Curie's own argument to establish the intrinsic symmetry of magnetic field. Consideration of the magnetic field caused by the current in a circular coil, and of electromagnetic induction, where a magnetic field occurs in the cause, leads us to ascribe an intrinsic symmetry (*d*) to magnetic field.

It is because symmetries can be established by appeal to a few simple experimental situations only that Curie's principle has content. Application of the principle will lead to restrictions on the possible outcome of any situation in electromagnetism other than those used to establish the symmetry of the electromagnetic quantities.

An assumption that was essential for the above deductions of symmetries was that charge can be treated as a scalar quantity. If, alternatively, we had taken magnetic charge as a fundamental, scalar, quantity then considerations similar to those above would have led to intrinsic symmetry (*c*) for the magnetic field H, and intrinsic symmetry (*d*) for the electric field E, and current or current density, j. Curie considered that processes which provide a link between electromagnetic and other types of phenomena, such as the thermo-electric effect, electrolysis and magnetic rotatory polarisation, force us into ascribing symmetry (*c*) to E and j and symmetry (*d*) to H.[1] This is not the case if we are prepared to accept sufficient alteration in our background theory. Let us take the thermo-electric effect as an example. Curie would have argued that since the cause is symmetric under a reflection by a plane through the wire joining the hot and cold junctions of the thermocouple, the effect must also show this symmetry. Electric current, then, which is the effect in this example, cannot have symmetry (*d*) according to this account of the phenomenon. But the argument is not valid if we allow the possibility that the material of the wire is not symmetric under reflection, as would be the case, for instance, if its fundamental components had a structure which is intrinsically left-handed or right-handed. We shall argue in a later section that it is quite feasible that charged particles such as the electron and proton have such a property. A reflection would transform them into their corresponding anti-particles.

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[1] Maxwell reached this conclusion by using essentially the same argument although he did not express it in quite the same terms or refer explicitly to a symmetry principle. His discussion appears in Chapter XXI of his *Treatise* (1954).

3 THE RELATION BETWEEN CURIE'S PRINCIPLE AND INVARIANCE TRANSFORMATIONS

Invariance transformations are usually construed as being applicable to the laws of nature. Any transformation of the co-ordinate system that leaves the mathematical form of a law unchanged is an invariance transformation for this law. In a deterministic theory, the laws of the theory enable the effect to be deduced from the cause. Within the framework of such a theory it can be shown quite simply and generally that Curie's principle must be satisfied for a transformation, T, if the laws of the theory are invariant under T. For if neither a cause nor the appropriate laws change under T, the derivation of the corresponding effect will take an identical form for the transformed and untransformed system and will yield identical expressions for the effect. In other words, the effect, too, is invariant under T and Curie's principle is satisfied.

The converse of the argument is illuminating, and indicates the alternative ways of approaching an apparent refutation of Curie's principle. If the proposed specification of a cause is symmetric under a transformation, T, and the corresponding effect is not symmetric under this transformation then we can conclude that either (1) the description of the cause and effect is incomplete or inadequate or (2) the laws connecting the cause and effect are not invariant under T and Curie's principle is refuted. Physical situations to which application of these considerations is instructive will be discussed in the next section. Before proceeding to this section we view invariance transformations from a slightly different standpoint derived from a paper by Houtappel *et al.* (1965) which makes it quite clear that within deterministic theories, Curie's principle is a particular consequence of the, more general, invariance properties of the theory.

The laws of a deterministic theory enable the effect, E, to be derived from the cause, C. We can write $E = f(C)$ where 'f' denotes a function which is known if the laws are known. The functional relationship is equivalent to a set (usually infinite) of ordered pairs $\langle C,E\rangle$ where the expression representing any possible cause in the theory appears as a first member of one of these pairs, the second member being the expression for the corresponding effect. This set of ordered pairs is an alternative formulation of the laws of the theory. Now, if T is an invariance transformation for the laws of the theory the set must remain unchanged under T. An invariance transformation will be a one to one mapping of the set onto itself. In general, any member of the set will be transformed into another member of the set under an invariance transformation, but Curie's principle is concerned with the special case of transformations which map each member of the set onto itself. For example, since classical mechanics is invariant under mirror reflections, the mirror image of a system of particles moving in accordance with the classical laws will also be consistent with these laws, while Curie's principle will be concerned with the subset of such systems which are unaltered by the mirror reflection.

4 SOME PHYSICAL SITUATIONS VIEWED IN THE LIGHT OF THE FOREGOING SYMMETRY CONSIDERATIONS

(i) *Oersted's experiment and the invariance of the laws of electromagnetic theory under inversion of the co-ordinate system.*

If a bar magnet lies N–S in the same plane, L, as a long straight wire carrying a current in an E–W direction it will experience a couple turning it out of the plane L. If, as was common at the time of Oersted's discovery (1820), we consider the magnet to have two poles, due perhaps to an accumulation of magnetic fluid at one of its ends and an opposite kind at the other, and if, further, we ascribe symmetry (*c*) to the electric current, then the above experiment constitutes a refutation of Curie's principle. The cause, the conjunction of the current and the magnet, is symmetric under a reflection by the plane L, whereas the effect, the couple acting on the magnet, changes its sense when reflected by L. Confronted with this situation two possibilities are open to us, either to concede that Curie's principle is refuted and that consequently the laws of electromagnetic theory are not invariant under a reflection of the co-ordinate system, or to re-examine and modify our description of the cause. If the principle is to be saved, either the current in the wire or the magnet must be asymmetric under the reflection in question. This can be achieved by retaining symmetry (*c*) for electric current and attributing the magnetic properties of the magnet to currents circulating within it in planes perpendicular to its axis. Historically, this was not the only path taken. For Oersted himself the required asymmetry resided in the current, it being regarded as a circulation of the "electric conflict" around the wire (see Tickner (1965), pp. 113–17), and several of the nineteenth-century aether theories of electromagnetism involved currents with symmetry (*d*) (see Whittaker (1951), vol. 1, ch. 9). The heuristic value of the principle is illustrated by this example. Not only did its application indicate the desirability of a change in the specification of the cause, but it also suggested the kind of change that was necessary.

It is interesting to reconsider the situation when the possible existence of magnetic monopoles is allowed. The force on such a pole in an Oersted type experiment would appear to violate Curie's principle unless the current or the magnetic pole is asymmetric under a reflection by the plane containing the pole and the current. We shall consider this problem from a more general point of view, in the light of the following discussion of some invariance properties of Maxwell's electromagnetic field equations.

We first note a mathematical distinction between two types of vector and two types of scalar quantities.

If a co-ordinate system is subjected to an orthogonal transformation, T, then a ***proper vector*** specified with respect to the co-ordinate system will also be subject to the transformation T, whether it is a proper or an improper transformation. In particular, if the co-ordinate system is inverted a proper vector V is transformed into $-V$. Proper vectors have symmetry (*c*). ***Pseudo-vectors***, on the other hand, while they transform like the co-ordinate system under proper orthogonal transformations, do not, in general, under improper orthogonal transformations. In particular, if the co-ordinate system is inverted, a pseudo vector remains unchanged. Pseudo vectors have symmetry (*d*).

> A. The curl of a proper vector is a pseudo vector and the curl of a pseudo vector is a proper vector.

We can also define pseudo scalars and proper scalars according to whether their sign is changed or unchanged under improper transformations.

> B. The divergence of a proper vector is a proper scalar, while the divergence of a pseudo vector is a pseudo scalar.

Let us now consider the behaviour of Maxwell's equations for free space under an inversion of the co-ordinate system. We have:

$$1.\ \text{Curl}\ H = \frac{1}{c}\frac{dE}{dt} + \frac{4\pi}{c}j \qquad 3.\ \text{div}\ E = 4\pi\rho$$

$$2.\ \text{Curl}\ E = -\frac{1}{c}\frac{dH}{dt} \qquad 4.\ \text{div}\ H = 0$$

In view of relation A it can be seen by inspection of equations 1 and 2 that, for these equations to be invariant under an inversion of the co-ordinate system, if E and j are proper vectors then H is a pseudo vector and conversely, if H is a proper vector, E and j must be pseudo vectors. If we regard charge as a proper scalar then, by relation B, equation 3 is invariant only if E is a proper vector, with j a proper vector and H a pseudo vector as a consequence. This is precisely the same conclusion that the treatment of charge as a proper scalar led us to in the application of Curie's principle to electromagnetic phenomena, since a proper scalar has symmetry (*c*) and a pseudo scalar symmetry (*d*). Equation 4 is invariant, trivially, for these symmetry assignments.

Suppose we now postulate the existence of magnetic monopoles and let the magnetic charge density be μ and the corresponding magnetic current density be g. We must now replace 2 by

$$2a. \quad \text{Curl } E = -\frac{1}{c}\frac{dH}{dt} - \frac{4\pi}{c}g,$$

and

$$4a. \quad \text{div } H = 4\pi\mu.$$

If the equations 1, 2*a* and 4*a* are to be invariant under inversion it is not possible for both ρ and μ to be proper scalars. The situation with respect to the invariance of the equations of electromagnetic theory under an inversion of the co-ordinate system can be summed up thus:

Either I*a*. E and j are proper vectors, H is a pseudo vector, ρ is a proper scalar, μ is a pseudo scalar and the equations are invariant under inversion.

or I*b*. H is a proper vector, E and j are pseudo vectors, ρ is a pseudo scalar, μ is a proper scalar and the equations are invariant under inversion.

or II. The equations are not invariant under an inversion of the co-ordinate system.

It is clear from this why Curie's principle must be violated when a magnetic monopole, which is regarded as unchanged by a reflection, experiences a force due to a current treated as a proper vector.[1] Modern interpretations of classical electromagnetic theory regard electric charge as a proper scalar and also regard Maxwell's equations to be invariant under an inversion of the co-ordinate system. This meets the requirements of I*a* provided magnetic charge is a pseudo scalar. It is interesting to note that the magnetic monopoles predicted by Dirac are indeed pseudo scalars although he does not explicitly state this.[1, 2]

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[1] The transformation which led to a violation of Curie's principle was a single reflection not an inversion. However, since an inversion can be achieved by a single reflection followed by a rotation and since all the equations under consideration are certainly invariant under rotations of the co-ordinate system, the equations are invariant under an inversion if and only if they are invariant under a single reflection.

(ii) *Optical Activity*

We consider a ray of plane polarised light incident on a cylindrically symmetric distribution of optically active liquid, the direction of the ray coinciding with the axis of the cylinder. If we describe the light in terms of classical electromagnetic theory and ignore the microstructure of the liquid there will be a plane, L, of symmetry passing through the ray. This plane will contain E or H depending on which is considered to be a proper vector. Since the plane of polarisation is rotated as the ray passes through the liquid, the effect, the emergent ray, will not in general be symmetric about L and Curie's principle appears to be refuted. The difficulty is removed by appealing to the microstructure of the liquid. The molecules of the optically active liquid have a structure which is not invariant under a mirror reflection, so that L is not a plane of symmetry for the cause.

This example is analogous to the apparent refutation of Curie's principle by the deflection of a bar magnet in a magnetic field. In both cases the apparent refutation arises from an inadequate description of the cause and is removed when the phenomena occurring in the cause are described in detail on a smaller scale.

(iii) *Weak interactions under inversion of the co-ordinate system*

We first note that the experiments which are taken as indicating that the law describing weak interactions is not invariant under an inversion of the co-ordinate system also appear to refute Curie's principle. The first of these experiments was performed by Wu *et al.* (1957). A cylinder of

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[1] See Dirac (1931) and Dirac (1948). Dirac's magnetic monopoles are evaluated by a line integral around a closed path. The sign of this integral will change if the co-ordinate system is subject to an improper transformation.

[2] A reason frequently quoted as indicating the likelihood or desirability of the existence of magnetic monopoles is that in some sense they would complete the symmetry of the theory. Thus Ford (1963, p. 122) claims "the magnetic monopole would bring to electricity and magnetism a symmetry that is lacking in our present picture" and Purcell (1965), p. 358, says "the world around us appears totally asymmetrical in the sense that we find no magnetic charges at all". Evidently, the symmetry referred to here is that of Maxwell's equations under an interchange of E and H, ρ and μ, and j and g. But unless magnetic monopoles (or alternatively electric charges) are pseudoscalars, their introduction leads to an asymmetry in electromagnetism, since in such a circumstance Maxwell's equations are no longer invariant under an inversion of the co-ordinate system. It would seem, from the literature, that this is not fully appreciated.

radioactive cobalt was placed in a magnetic field so that the latter was directed along the axis of the cylinder. The cobalt was cooled so that the interaction between the cobalt nuclei and the field was appreciable compared with the thermal agitations. If the magnetic field is regarded as a pseudo vector then this arrangement is symmetric with respect to a plane bisecting the cylinder of cobalt and perpendicular to its axis. It was found experimentally that the distribution of the intensity of β-rays emitted by the radioactive cobalt nuclei was not symmetric under reflection by this plane. If this description of the experiment is accepted then Curie's principle is violated and the law of interaction between the β-particles and the parent nuclei is not invariant under an inversion of the co-ordinate system.

But the discussion of example (i) indicated that we have at least one alternative description of this experiment. We can regard H as a proper vector and, as a consequence, charge as a pseudo scalar. With this interpretation no improper transformation is a symmetry transformation of the cause since all protons will be transformed into antiprotons and all electrons into positrons. Further, any reflection of the system, although not a symmetry transformation, does result in a cause and effect consistent with known physical laws. If charged particles were characterised by a structure such that the mirror images of electrons and protons were equivalent to their corresponding anti-particles, then we could consistently regard E as a pseudo vector, H as a proper vector and magnetic monopoles, if they exist, as proper scalars.[1] The laws governing β-decay would then be invariant under inversions of the co-ordinate system and Curie's principle would not be refuted by these processes.

In one respect the saving of Curie's principle in this way is analogous to the way in which it was saved from refutation by optical rotation. In the one case, charged particles, and in the other, the molecules of the optically active liquid, were regarded as having a structure which is changed by a mirror reflection. But there is a significant respect in which the two cases differ. Ever since 1847, when Pasteur prepared two types of tartaric acid and observed their crystal structures to be mirror images of each other, there has been strong evidence, apart from rotation of the plane of polarisation of light, for an inherent right hand or left hand structure in optically active liquids. At present there is no strong evidence of this sort for an inherent structure of charged particles.

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[1] Although, as we have already noted, magnetic poles are pseudo scalars and electric charge is a proper scalar in Dirac's theory, he does admit that the roles of the two can be interchanged. To quote from his 1948 paper (p. 830), "The theory developed in the present paper is essentially symmetrical between electric charges and magnetic poles . . . one could work equally well with the roles of the charges and poles interchanged".

(iv) *α-Decay*

The previous three examples involved effects which followed from their causes according to deterministic laws. There are many quantum events which are not causal in this way, according to present day quantum mechanics. For example, the time at which, and the direction in which, an α-particle is emitted from a radio-active nucleus is not precisely predictable in quantum mechanics. If we regard a nucleus before decay as spherically symmetric, the subsequent emission of an α-particle in a particular direction seems to constitute a refutation of Curie's principle. But the situations before and after the emission do not qualify as cause and effect as we have defined them, since the validity of the cause is not sufficient to ensure the validity of the effect. If there were available a dynamic description of the structure of the nucleus which was sufficient, according to some law, to ensure the emission of an α-particle at the appropriate time and in the appropriate direction, then Curie's principle would be satisfied for a transformation if the law was invariant under the transformation. The probability distribution for the direction of emission of an α-particle from a nucleus in a given state is predicted by quantum mechanics, and this distribution is spherically symmetric for a nucleus in an initial S state, consistent with Curie's principle.

5 THE CAUSE-EFFECT ASYMMETRY OF CURIE'S PRINCIPLE

Curie's principle is not invariant with respect to an interchange of the terms 'cause' and 'effect'. Some considerations seem to indicate that the asymmetry arises only because irrelevant detail is included in the description of the cause. For example, if a description of irregularly shaped insulation around the current carrying wire is included in the analysis of an Oersted-type experiment, then the effect, the magnetic field distribution, is more symmetric than the cause, whereas the cause and effect have the same symmetry if reference to the insulation is omitted. In this section we show that the cause-effect asymmetry cannot always be removed in this way for two reasons, one arising from a convention in our terminology and another arising, more interestingly, for physical reasons.

The first arises when the cause and effect are descriptions of different aspects of a system at the same time. As an example we can take a short straight wire moving so as to cut magnetic lines of force at right angles as our cause and the induced e.m.f. as the effect. This effect is more symmetric than its cause. In the situation described here the validity of the cause is a sufficient condition for the validity of the effect but the reverse is certainly not the case. The existence of an e.m.f. along the length of a wire is clearly not sufficient for the wire to be moving at right angles to a magnetic field. The asymmetry in the principle arises because, although we hope that a cause is a sufficient condition for the corresponding effect we do not demand that it be a necessary condition. There may be many possible causes for a particular effect, but only one effect for an adequately specified cause.

The second reason for the cause-effect asymmetry applies in those instances where the terms 'cause' and 'effect' describe a complete system at different times. Suppose a system to be described by D_1 at time t_1 and by D_2 at time t_2, where $t_2 > t_1$. In a deterministic theory the validity of the cause, D_1, is sufficient to ensure the validity of the effect, D_2. According to Curie's principle D_2 cannot be more asymmetric than D_1. We now consider the possibility of a system described by D_2 being a sufficient cause for the system D_1. If such a situation were possible we could then conclude from Curie's principle that D_1 could not be more asymmetric than D_2, which together with the original condition implies that the symmetry of D_1 is *the same* as that of D_2. This situation arises if the laws connecting D_1 and D_2 are invariant under a time reversal. The laws of thermodynamics are not invariant under a time reversal and there are many examples of an increase in symmetry. For example, we can imagine an irregular block of ice enclosed in a spherical container which after some time melts and then eventually evaporates, so that the final state is a spherical distribution of gas. The effect is clearly more symmetric than the cause, and Curie's principle requires that the reverse process be impossible.

6 STATUS AND USE OF THE PRINCIPLE

Given a conceptual framework in which a class of physical situations can be described, Curie's principle has predictive power in so far as it places restrictions on the outcome of these situations. This is the case whether or not the precise nature of the laws governing the processes involved are known. If deterministic laws are known, then the principle will hold for all transformations under which these laws are invariant. On the assumption that the descriptions of a particular cause and its corresponding effect are correct and adequate, it is possible for some physical process to constitute a refutation of Curie's principle for some transformation, T, in which case any law connecting the cause and the effect so described will not be invariant under T. But if we wish to protect the principle against such refutation, we always have the option of postulating some deficiency in the description of the cause and its effect. It is because of this option that uses of Curie's principle are not completely subsumed under considerations of the invariance properties of physical laws. A refusal to give up the principle can be fruitful in guiding us towards new theories. The discussion in section 4 shows how application of the principle to Oersted's experiment not only indicates an inadequacy in the description of a magnet in terms of magnetic poles but also suggests that a correct description is such that symmetry (*d*) rather than symmetry (*c*) be ascribed to bar magnets. To give a more modern example, those β-decay processes which, according to the conventional description, violate Curie's principle no longer do so if it is postulated that electrons and positrons are distinguished from each other by characteristic structures which are mirror images of each other. Physics may reach a stage of development allowing such a conjecture to prove fruitful. We must reject Curie's remark that the principle "prevents us from being misled into the search for unrealisable phenomena" (1894, p. 413). Such a conservative attitude would be a hindrance to the making of many new discoveries such as that of a magnetic field due to a linear current and the asymmetries associated with processes involving weak interactions.

On a somewhat lower level of importance, Curie's principle can be useful, within some accepted theoretical framework, for checking analytic solutions of problems and for obtaining partial solutions to complicated problems without solving them analytically. Thus, without any detailed analysis we can conclude that any e.m.f.s induced in a cylindrical bar magnet subject to a torsional twist must possess the symmetry characteristics common to the magnet and the torsional twist.

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Erotetic Logic and the Structure of Scientific Revolution

by SCOTT A. KLEINER

RECENT CRITICS of the programme of logical empiricism have made the important and doubtlessly true claim that rigorous formalisation of existing scientific theories is insufficient for an adequate philosophical account of the scientific enterprise.[1] However, some of these critics have also condemned clarifying the content of a theory by axiomatising the theory as being either impossible or unenlightening. (Kuhn (1962), p. 46 and Hanson (1961), p. 1.) In what follows I hope to show by example that conceptual clarification by axiomatisation is possible and that its achievement can be enlightening insofar as it can contribute to the solution of several important problems left open by these critics.

1 IN ADDITION to a few elementary points of first and second-order propositional logic, I will need as tools a few principles of *erotetic logic*, the logic of questions. These principles need be represented only informally.

Three types of question will interest me here. The reasons for selecting these types will be made clear later. For my purposes, the most important kind of question is that in which the *questioner* (he who poses the question) demands of his *audience* (any person to whom the question is addressed) that they supply a predicate or a relation for a given object, pair of objects, etc. I will call this kind of question a *what-question*. For example, 'What is the colour of *a*?' is such a question, where *a* is an individual name standing for some presumably coloured object. I will also later have occasion to mention other types of question: The request for an audience's affirmation or denial of a given sentence can be expressed by a *whether-question*, e.g. 'Is *a* red?'[2] Finally, *why-questions* are frequently used as requests for facts by virtue of which a given situation or event that has

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[1] For example, Hanson (1961), Kuhn (1962), and Toulmin (1961) have taken the position that a study of the history of science will reveal the *pragmatics* (the nature of the activities characteristic of the practice of) a given science, where pragmatics has been neglected by the proponents of logical empiricism.

[2] The grounds for the distinction between what- and whether-questions, the import of questions in the history of western logic, and some informal principles of erotetic logic are discussed in Prior and Prior (1955).

occurred should be expected to occur, i.e. requests for *explanations* (cf. Bromberger, 1966).

Like statements, requests and commands, questions may be and are accepted or rejected, preferably with some sort of justificatory argument. We accept or reject questions as we acknowledge or reject the obligation they place upon us, not as we judge their truth or falsity as in the case of statements.

We might reject or refuse to answer a what-question because it is *unsound*, that is, because it has a false presupposition. 'What is the colour of *a*?' where '*a*' refers to the number four, or a photon, is unsound because numbers and photons have no colour. Similarly, 'Is the colour of *a* red?' presupposes the truth of the same sentence, '*a* is coloured'. 'Why is the colour of *a* red?' presupposes not only the truth of the latter sentence, but also an affirmative answer to the corresponding whether-question. *A fortiori*, if *a* has no colour, both 'Is the colour of *a* red?' and 'Why is the colour of *a* red?' are unsound.

Also, any of these three question-types is unsound if its presupposition is *syntactically improper* (if it is ungrammatical). 'What is the size of *a*?', 'Is *a* bigger?' and 'Why is *a* bigger?' apart from any indication that *a* is being compared with another object *b*, are all unsound. Their presuppositions, '*a* is of size *N*', '*a* is bigger' are proscribed by the syntax of scientific English and hence cannot be true (or false). By contrast, 'What is the size of *a* relative to *b*?' is sound provided *a* and *b* are extended and '*a* is *N* (a rational number) times *b* in size' is syntactically proper.

That a question is *indefinite* is also a good reason for refusing to answer it. If the questioner has no idea of what he might accept as an answer to the question, and hence is unable to express to his audience what will satisfy him, the audience will be incapable of *acting* so as to produce a satisfactory answer. They might guess something that happens to silence him, but this latter response is more like accidentally turning off a noisy radio than answering a question. Where an audience cannot act it is released from the obligation to act. In this case it is released from an obligation to answer a question that cannot be answered, an indefinite question. Notice also that a question is either definite or indefinite; there are no intermediary degrees.

What-questions are indefinite when the questioner cannot or does not specify or otherwise indicate what kind of predicate or relation will satisfy his demand. 'What is *a*?' is indefinite apart from any context that might indicate that colour, relative size, mass, physical type, etc., is being asked for.

We might also be justified in rejecting a question because it does not meet justified or justifiable standards of *precision.* The precision of a what-question depends upon the precision of its presupposition. The expression 'What is the colour of *a*?' can be used to express two or more distinct questions depending upon the context in which the expression is used. This context will include the questions' presuppositions, and what-questions presupposing '*a* has a place *X* on a continuous colour spectrum', are more precise than similarly expressed what-questions presupposing '*a* is scarlet, red, orange, yellow, . . . etc.' (through the usual list of colour names in ordinary English). The latter question is appropriate in many contexts belonging to daily life, but the optical spectroscopist is surely justified in rejecting the latter in favour of the former question in contexts constituting his research activity.

The following problem might occur to the reader: Are the questions 'What is the colour of *a*?' (A) under the presupposition that *a* has a place on a continuous colour-spectrum and (B) under the presupposition that *a* has one of the properties mentioned by ordinary English colour-words the same question? Yes and no. Yes, in the sense that in both (A) and (B) we are presented by ostension or some other means of reference with the same object *a*, and a classification as to colour is demanded or requested. Also, possible answers to (B) fall within the scope of the possible answers to (A), but not conversely. No, because the possible answers to (B) form a proper subset (are included in but not identical to) those to (A). The difference in the extension of possible answers to (A) and (B) is a consequence of the different conceptual background presupposed by each. If (A) is made as precise as we know how, its presuppositions will include axioms of real number theory. On the other hand, (B) is most precise if its presuppositions include explicit statements of assumptions implicit in the use of colour-words in everyday life. These assumptions include principles such as 'No object can have at the same time two colours all over its surface', and 'All coloured objects are extended'. In the case of (B) the relation between explicitness of presupposition and precision of question is easily seen: both principles place necessary conditions (restrictions) on possible answers to (B). Further philosophical analyses of the concept of colour might well extend this list of assumptions, and the list can be regarded as complete when the listed principles taken together provide sufficient conditions for having colour.

To make precise any specific application of the first-order predicate calculus (QC–1), we must specify the range of its variables as well as assign interpretations to its names, predicates and relations. We may specify the range of these variables by stating necessary and sufficient conditions for membership in a set, where the set is taken to be the range of the variables. For example, (QC–1) may be interpreted in elementary number theory by taking 'identity' and 'is greater than' as dyadic relations, the individual constants '*a*', '*b*' . . . as names of integers, and the set of positive integers N^+ as the range of the individual variables. Peano's axioms then state necessary and sufficient conditions for membership in N^+.

Once we have specified the range of the variables (the domain of discourse) in this particular interpretation of (QC–1), we know the kind of object to which the open formula $x+2y = 4$ applies, viz. the elements of N^+. Yet the open formula alone is insufficient to convey a specific application. It does not indicate of which object-pair, $\langle 2,1\rangle$ or $\langle 0,2\rangle$, etc., it holds. If this formula is written down or stated with the intent of demanding of an audience that it supply a pair of objects satisfying it, it is being used to express a what-question. In our example here the what-question is a problem in a restricted numerical algebra. As long as the questioner is aware that possible answers fall only within the set $N^+ \times N^+$ (the cartesian product of N^+ by itself) he is capable of making this question perfectly definite and precise.

The points I have made regarding the definiteness and precision of what-questions can be put in a form that will be useful later in this essay if we extend the remarks in the last paragraph to applications of the second-order predicate calculus (QC–2), i.e. the branch of logic in which we consider predicate and relational variables as well as individual variables. 'What is the colour of *a*?' can now be expressed by 'What *X* is *a*?' or just the open formula '*Xa*', where by the syntax of (QC–2) '*X*' ranges over a yet to be specified set of predicates. The question is indefinite unless this latter set is specified. If the set is the finite set of colour-predicates available to the plain man using plain English, then '*Xa*' can express the less precise question (B). If '*X*' ranges over a continuous colour-interval then we have the more precise question (A).

2 IN HIS IMPORTANT BOOK, *The Structure of Scientific Revolutions*, T. S. Kuhn (1962) distinguishes two phases of research activity appearing in the "advanced" sciences such as physics, astronomy or chemistry. *Normal science* is research guided by an established *paradigm* (Kuhn (1962), p. 10). A paradigm consists of more or less explicitly and precisely formulated theoretical principles and beliefs about what are (i) their most successful applications, (ii) the standards for their successful application, (iii) the most important problems demanding further scrutiny, (iv) the most promising methods of research and (v) the most useful measuring techniques and instruments (Kuhn (1962), pp. 10, 11). *Extraordinary*

science is research in the absence of a generally accepted paradigm, where a common paradigm is absent either because no paradigm has been sufficiently successful to induce its general acceptance (as is the case for less "advanced" sciences), or because researchers in a given field have become sceptical about the tenets of the established paradigm (Kuhn (1962), secs. II, VIII). This scepticism might be generated by the failure of repeated efforts to solve some problem recognised by the paradigm.

In Kuhn's view, a *scientific revolution* is just the rejection of one and the acceptance of a different paradigm. However, as recent critics (Shapere, 1964) have pointed out, the criteria for paradigm-difference Kuhn explicitly and implicitly adopts make every slight modification of law, theory, application or instrumentation revolutionary. Kuhn's claims that revolutions within specialised fields can hardly be expected to be publicly visible does not relieve us of certain implausible consequences of his theory (Kuhn (1962), p. 92). If instrumentation is an essential component of a paradigm, one such consequence is that revolutionary scientific changes include the adoption of a newly invented instrument for measuring an old quantity, such as length. Similarly, an extension of an established theory without internal revision to some newly discovered phenomenon is just as revolutionary as the modification of one or more of the theory's fundamental principles. As to the latter case, Kuhn claims that the subsumption of X-rays under Maxwell's electrodynamics had the same revolutionary effect upon radiation theory as did Laviosier's discovery of oxygen on chemistry and Einstein's special theory of relativity on mechanics.

The implausibility of these consequences comes out sharply in view of Kuhn's claims about the effects (or consequences; he is not clear which) of holding different paradigms. Proponents of different paradigms cannot settle their differences by observation and argument. Both view completely different physical situations as problematic, and, at that, as problematic in different ways. Also, their standards for scientific achievement are completely different (Kuhn (1962), p. 102). In fact, 'sameness' and 'difference' of physical situation do not have the usual meanings for proponents of competing paradigms. The "worlds" in which they live cannot be related to one another by the usual means of relating physical circumstances or temporal and spatial locations.

Clearly, tinkering with minor assumptions, altering some of the standard research procedures or instrumentation, or the unexpected extension of the scope of a theory cannot have such drastic consequences. It is highly doubtful that even the most comprehensive scientific revolutions, e.g. that from Aristotelian to Newtonian physics and cosmology, have such drastic effects (Shapere, 1966).

Yet certain elements of Kuhn's position have their plausibility. It is quite clear that different problems (or no problem at all) can be raised about the same physical situation or the same phenomenon presented to our senses. For example, to some the changes in the configuration of celestial objects require no quantitative description. Points of light just come and go as do meteors, shooting stars and clouds. To others these occurrences demand quantitative description, e.g. in terms of periods between risings and settings, or between successive coincidences with a point of reference in the zodiac. To some the motions of celestial bodies just occur and require no explanation. To others, the motions must be subsumed under a theory including force-laws and laws of motion.[1] Also scientific progress is contingent upon researchers conducting their investigations in one rather than another direction. A little reflection on what goes on in conducting an elementary experiment in the physics laboratory shows that, e.g. the temperature of an object cannot be obtained by passive attention to the "data" of our senses. Also, it is unlikely to be obtained unless a specific procedure or sequence of actions is followed.

Thus, it appears that the way to overcome the above-mentioned difficulties in Kuhn's theory of scientific revolution is by clarification and revision, not by outright rejection.

3 IN THE USUAL TEXTBOOK exposition of classical particle mechanics (CPM) we will find laws mentioning configurations, masses, forces, velocities, accelerations, etc., and perhaps also non-mechanical quantities such as electric charges, gravitational masses and magnetic moments. Also there frequently appear "definitions" of one or more of these quantities in terms of either other such quantities or measuring operations. Finally, the exposition will include deductive arguments leading to specific applications of the theory.

Recently it has been shown that a number of physical theories can be represented in axiomatic form within set theory (McKinsey (1953) and Bunge, 1967), or, equivalently, can be represented as set-theoretical predicates such as 'is a system of classical particle mechanics', 'is a system of relativistic particle mechanics', etc. (Suppes (1957), ch. 12). These representations are not full formalisations in which the mathematical background of a theory is explicitly axiomatised. The formidable task of formalisation is avoided by stating simply that the set theoretical framework to which physical axioms are added includes whatever is needed from logic and mathematics, e.g. the predicate calculus with identity, the theory of real or complex numbers, functions of real numbers, derivatives and integrals of these functions.

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[1] For the ancient Greek Heraclitus the celestial bodies were destroyed and re-created every day. Therefore these objects have no permanent configuration and cannot be said to move from day to day. All celestial objects, in Heraclitus's view, are 'wanderers.' However, in the mathematical astronomy founded by the Pythagoreans, popularised by Plato and described by Aristotle, the determination of quantitative parameters for the superimposed spherical motions of the sun, moon and the planets is the concern of astronomy. Causes of change are inappropriate objects of discussion in ancient Greek astronomy. In Newton's time the behaviour of celestial objects is scrutinised quite differently. Here the problem-field regarding the behaviour of celestial objects is much the same as that regarding the behaviour of falling bodies, projectiles, pendulums, elastic bodies and fluids. For an excellent account of ancient Greek astronomy and the transition to modern astronomy, see Dreyer (1953).

However, what is important for my purposes here is that these axiomatic representations contain an explicit catalogue of expressions needed for the statement of laws and applications of the theory. In particular, the extra-logical symbols with appropriate physical interpretations (a *vocabulary*) and grammatical rules governing these symbols (a *proper syntax*[1]) must be made fully explicit in the representation of the theory.

There are certain advantages to having a theory in axiomatic form. Axiomatisation makes it possible to demonstrate that 'force', 'mass', 'position', and 'time' are all independent notions in CPM. Furthermore, the explicit syntax of the axiomatic representation makes precise certain limitations regarding what can be said and what can be asked within the framework of the theory. This last feature has been overlooked by recent critics of the formal approach to the philosophy of science. It is my concern in the remainder of this essay to show that an appreciation of this feature will advance the enterprise launched by these critics.

To illustrate this point I will consider the following axioms constituting part of the definition of the predicate 'is a system of classical particle mechanics'

CP1: The set Σ is non-empty $[(\exists\sigma)(\sigma \in \Sigma)]$.

Σ is a set over which range the variables $\sigma, \sigma', \sigma'', \ldots$ and the members of this set are denoted by the individual constants $\sigma_1, \ldots, \sigma_n$. Σ is the set of classical material particles, and it includes any macroscopic massive body whose rotation is not taken into account.

CP2: T is an interval of the set of real numbers $[T \subseteq R]$.

The set T is the range of the time variable t appearing in, e.g. expressions for position or velocity in classical particle mechanics (CPM). The boundaries of T, viz. t_0 and t, are selected as is convenient for the problem at hand. Metric postulates for time are part of the classical theory of universal time (Bunge (1967), pp. 94 f.) and may be regarded as part of the formal background for CPM.

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[1] I choose 'proper syntax' to differentiate between (i) rules in the overall logical framework governing symbols, the formation of terms, expressions, formulae, sentences and proofs and (ii) assumptions peculiar to a particular theory regarding the syntactic classification of non-logical symbols. ('Proper syntax' corresponds to 'proper axiom' in this respect.) Specific non-logical terms in a theory such as CPM are given a physical interpretation by semantic rules associated with the theory. What I call proper syntax classifies syntactically terms that already have a physical interpretation. Hence the presuppositions constituting the proper syntax of a theory include the semantic rules assigning a physical interpretation to the theory's non-logical symbols.

> CP3: (a) S is a vector-valued function of objects and moments of time [$S: \Sigma \times T \rightarrow R^3$].
>
> (b) S is twice-differentiable with respect to time.

$S(\sigma,t)$ is the position, $\dot{S}(\sigma,t)$ the velocity and $\ddot{S}(\sigma,t)$ the acceleration of particle σ at time t. The topological and metric postulates of Euclidean geometry that govern S are, as in the case of T, taken to be included in CPM's formal background.

> CP4: M is a function of objects into the positive real numbers [$M: \Sigma \rightarrow R^+$].

$M(\sigma)$ is the mass of particle σ.

These postulates tell us, e.g. that every $\sigma \in \Sigma$ at a specific $t \in T$ has a unique position, velocity, and acceleration and throughout T a unique mass. Insofar as the language of classical particle mechanics can be applied to the world, a fact for which the empirical evidence is indeed impressive, its proper syntax reflects the world's structure. These postulates are conceived, elaborated and revised in view of their applicability to the world, and one can argue on empirical grounds for or against them. Hence they have empirical import.

To make this last point more concrete, if nature consisted of fundamental atomic components all having equal masses, then M could not be real-valued, but rather would take values in the set N^+ of positive natural numbers. Contrary to the claim made in CP4, masses would be construed as ratios with the fundamental mass taken as denominator, thereby making all possible mass-values integral. But we believe that there are no such fundamental masses, and our belief is well-supported by evidence from atomic and nuclear physics.

The postulates also provide explicit limitations on what counts as a proposition in CPM, or equivalently, of what expressions we may raise a whether-question. For example, the expression

$$S(\sigma_1,t_0) = \langle s_1,s_2,s_3 \rangle \tag{1}$$

where '$\langle s_1,s_2,s_3 \rangle$' names an element (a vector) in R^3, by CP4, can be either true or false depending upon the position in fact occupied by σ_1 at t_0. However, the expressions

$$S(\sigma_1) = \langle s_1,s_2,s_3 \rangle \tag{2}$$

and

$$M(\sigma_1,\sigma_2,t_0) = m_1 \tag{3}$$

are not legitimate propositions in CPM. Neither can be true or false and neither can be the object of a whether-question.

In a parallel way these postulates constrain the questions that can be raised in the framework of CPM. The expressions

$$S(\sigma_1,t_0) = \langle x_1,x_2,x_3\rangle \tag{4}$$

and

$$M(\sigma_1) = m \tag{5}$$

where '$\langle x_1,x_2,x_3\rangle$' and '$m$' are variables ranging over R^3 and R^+ respectively, assign an indefinite position to σ_1 at t_0 and an indefinite mass to σ_1. However, the postulates tell us what kind of constants fill the places marked by these variables. Hence, in the context of our postulates, (4) and (5) are suitable for posing what-questions. They are suitable and precise expressions for the questions 'What is the position of σ_1 at t_0?' and 'What is the mass of σ_1?' But expressions generated from (2) and (3) by replacing '$\langle s_1,s_2,s_3\rangle$' and '$m_1$' by the corresponding variables cannot express sound what-questions in the context of CPM. The presuppositions of these latter questions are not propositions in CPM and cannot be true (or false), and thus the questions are unsound.

Notice that our postulates have left out any mention of a material reference frame. This omission is in keeping with the classical belief that position, velocity, acceleration and mass are "absolute" rather than "relative" quantities. That is, the condition of any other object in the universe has no bearing on the mechanical characteristics of a particle. This belief is built into CP3 and CP4, and is the basis for rejecting statement (3) as violating CPM's syntax. Without this belief we might have allowed another variable in the domain of S or M, namely one ranging over the possible conditions of one or another component of the universe distinct from the object σ_1.

By contrast, the proper syntax for special relativity theory (SRT) includes an explicit rejection of the concepts of absolute position, velocity and mass. The position of an object depends upon the object's motion relative to a material reference frame. That is, position takes one value when the object in question and the reference frame are at rest and a different value when object and frame are in motion relative to the observer. Similarly, relativistic mass will depend on the object's motion.

Also, SRT mentions light signals (bursts of electromagnetic radiation) as well as material (massive) particles. For example, the principles attributing the limiting velocity c relative to all inertial frames refers to all physical objects, including light signals (see SR8 below).

Thus we should expect the proper axioms for STR to differ in several ways from those for CPM. The following statements (adopted from Bunge (1967), p. 185) should illustrate this difference:

SR1: The set Σ is non-empty $[(\exists\sigma)(\sigma\epsilon\Sigma)]$.

$\sigma_1,\ldots,\sigma_n$ are values of the variables $\sigma, \sigma', \sigma'',\ldots$ and name physical objects in Σ. The set Σ includes massive bodies moving near the speed of light as well as light signals.

SR2: The proper subset Ξ of Σ is non-empty $[(\exists\sigma)(\sigma\epsilon\Xi) \wedge (\Xi \subseteq \Sigma)]$.

Ξ is the set of light, radio, or other electromagnetic signals. In view of postulates SR5 below, Ξ can be interpreted as the property of being a point of constant phase on an electromagnetic wave front.

SR3: The proper subset K of Σ is non-empty and excludes Ξ
$[(\exists\sigma)(\sigma\epsilon K) \wedge (K \subseteq \Sigma-\Xi)]$.

K is the set of inertial reference frames.

SR4: T is an interval of the set of real numbers $[T \subseteq R]$.

T is a real interval, as in CPM. However, according to the metric postulates for relativistic chronology (Bunge (1967), pp. 95 f.), the values taken by t depend upon the inertial reference frame $\sigma'\epsilon K$ chosen as well as upon the initial moment t_0. For simplicity I will stipulate that the value of t is determined in the same frame in which the behaviour of the elements of Σ are considered in a particular problem. Thus T is the range of values for local time t in frame $\sigma'\epsilon K$.

SR5: (a) S is a vector-valued function of physical objects, inertial frames and moments of time $[S\colon \Sigma\times K\times T \to R^3]$.

(b) S is twice-differentiable with respect to time.

If $\sigma'\epsilon K$, $S(\sigma,\sigma',t)$ is the position, $\dot{S}(\sigma,\sigma',t)$ the velocity and $\ddot{S}(\sigma,\sigma',t)$ the acceleration of physical object σ with respect to frame σ' at time t.

SR8: (a) The maximum velocity of any material particle in any inertial reference frame is less than the velocity of light

$[(\forall\sigma)(\forall\sigma')(\forall t)(((\sigma'\epsilon K) \wedge (\sigma\epsilon\Sigma-\Xi)) \supset (\dot{S}(\sigma,\sigma',t) < c))]$.

(b) The velocity of any light signal in any inertial reference frame is always the same constant value

$(\forall\sigma)(\forall\sigma')(\forall t)(((\sigma'\epsilon K) \wedge (\sigma\epsilon\Xi)) \supset (\dot{S}(\sigma,\sigma',t) = c))$.

SR9: M is a function of material particles, inertial frames and time into the positive real numbers $[M\colon \Sigma-\Xi\times K\times T \to R^+]$.

If $\sigma\epsilon\Sigma-\Xi$, $\sigma'\epsilon K$, $M(\sigma,\sigma',t)$ is the relativistic mass of material particle σ in frame σ' at time t. M is not defined for electromagnetic signals and, in a given frame, can depend upon the velocity of the material particle for which it is defined. Since the velocity in a given frame can depend upon time, M also is a function of time. Also, if $\sigma'\epsilon K$, $\sigma\epsilon\Sigma - \Xi$ and $\dot{S}(\sigma,\sigma',t) = (0,0,0)$, then $M(\sigma,\sigma',t) = m_0$, the rest mass of σ.

These postulates do not exhaust those needed to make the descriptive language of SRT fully explicit, but they will suffice to show that some statements and questions possible in SRT cannot be made or raised in CPM and conversely. Statement (3) above satisfies the syntax of STR, as can be seen from SR9. Hence, the whether-question 'Is m_1 the mass of σ_1 which is in inertial frame σ_2 at t_0?' is sound in STR but not in CPM. Also, a number of what-questions are sound in SRT but not in CPM:

$$M(\sigma_1,\sigma',t_0) = m_1 \tag{6}$$

with $\sigma'\epsilon K$ can express 'In what frame does σ_1 have mass m_1 at t_0?' Similarly,

$$S(\sigma_1,\sigma_2,t_0) = \langle x_1,x_2,x_3\rangle \tag{7}$$

can express 'What is the position of σ_1 in frame σ_2 at t_0?' Conversely the question expressed by (4), 'What is the position of σ_0 at t_0?' cannot be given a definite answer in STR because $S(\sigma_1,\sigma',t)$ will take different values for different values of $\sigma'\epsilon K$. 'Position' is indeterminate in SRT unless an inertial reference frame is specified, and without specifying the latter object any question asking for position is unsound.

Turning now to quantum mechanics (QM), we should recall that in the early stages of QM's development the belief that the same object could have wave- and particle-like attributes appeared wholly paradoxical to most physicists and philosophers. That an object, e.g. an electron or a photon, is spread over a finite region of space[1] appears incompatible with the assumption that the object bears mass, energy and momentum all concentrated at a given moment at one point. Also, although the point-like concentration of mass, momentum and energy is compatible with the descriptive postulates of CPM and STR, the spatial spread of quantum-objects violates both CP3 and SR5, by which classical and relativistic material bodies have a unique position.

QM's descriptive language departs even more radically from CPM's than does STR's. A full and adequate account of quantum descriptions would require the resolution of a number of yet unsettled issues regarding the foundations and interpretations of QM. However, I propose here only to say enough to show that the proper syntax and the statements and questions possible in QM (at least in Bohr's 'Copenhagen Interpretation' described by Feyerabend, 1968) differ considerably from those in CPM and

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[1] This region has dimensions of the order $\lambda = h/p$, where p is the object's momentum and h is Planck's constant, of value $h = 6{\cdot}63 \times 10^{-34}$ joule-seconds.

STR. The following postulates are adopted with some modification from Bunge (1968). They are added to a logical apparatus including postulates for Hilbert spaces, probability theory, Euclidean geometry, and classical universal time.

QM1: The set Σ is non-empty. $(\exists\sigma)(\sigma\epsilon\Sigma)$.

Σ is the set of material bodies whose dimensions, mass and velocity fall respectively within the order of 10^{-8} cm, the order of 10^{-24} gm and the interval $[0,v,]$ where $v \nleq c$, c being the velocity of light.

QM2: The set $\bar{\Sigma}$ is non-empty $[(\exists\bar{\sigma})(\bar{\sigma}\epsilon\Sigma)]$.

$\{\bar{\sigma}_1,\ldots,\ \bar{\sigma}_n\} = \bar{\Sigma}$ is the set of physical objects forming a context for the quantum-particle σ. $\bar{\Sigma}$ can include other quantum particles, measuring instruments and force-fields.

QM3: (a) Ψ is a function of quantum-particles, physical context, location and time into an interval of Hilbert space

$[\Psi\colon\ \Sigma\times\bar{\Sigma}\times\{X\}\times T\rightarrow H]$.

(b) For every quantum-particle-context pair, Ψ, $\Delta\Psi$, and $d\Psi/dt$ are finite and continuous throughout space and time.

(c) $\iiint \Psi^*\Psi\, dxdydz$ exists and is finite in some region of space.

(d) $\Psi^*\Psi\Delta x\Delta y\Delta z = P$, where P is a function onto the real interval $[0,1]$ and includes in its domain sets of quantum-particle-context pairs, location and time $[P\colon\Sigma\times\bar{\Sigma}\times\{X\}\times T\rightarrow[0,1]]$.

$P(\sigma,\bar{\sigma},t)$ is the probability that σ in context $\bar{\sigma}$ will be found at time t in the region $\Delta x\Delta y\Delta x$, located approximately at X. A particle's occupying this region approximates the classical position S for a quantum particle. Note that X is an ordered triad of real numbers assigned to each point of space and is distinct from particle-coordinates S.

The first point to be noticed here is that we speak of the condition of a quantum-object only relative to a specified context. In QM we cannot, as in CPM and SRT, ask for the position, velocity, momentum or total energy without specifying the context (which might, in a particular case, be empty or null). The quantum-object's interaction with its context determines the states accessible to it. For example, the energy-states accessible to a free electron (where the context contains no material objects or null-valued forces) are continuous, but for an electron bound in the orbit of an atom, they are discrete. Whether or not a quantum-object has a determinate position, momentum or energy will depend upon its preparation and the interaction by which these quantities are measured. A quantum-object prepared in a determinate-position state (e.g. by passing through a diffraction-slit) and appropriately measured (by scattering sufficiently high-energy photons) will maintain its precise position (provided that the time lapse between preparation and measurement is sufficiently short), but its momentum in this case will be indeterminate.

Thus, by relativising in QM3 the condition of σ to the context $\bar{\sigma}$, classical what-questions such as 'What is the position (momentum, etc.) of σ_1 at t_0?' are ruled out of QM as unsound. We cannot assume in general that quantum-objects always or in all contexts have a unique position, momentum, or total energy.

Also, a term appears in QM that has no place in CPM or STR, viz. P, the probability that σ in the context $\bar{\sigma}$ at time t is in the spatial region ΔV. Probability has no place in the problem-fields of CPM and STR. Although 'probability' might appear in the discussion of error in the latter two theories, their assumptions that position, mass, etc. are real-valued entail that error is believed in principle eliminable from all descriptions formulated within their framework. That is, 'probability' as appears in the discussion of observations in classical contexts is a measure of the adequacy of our cognition of a fact, and is not part of the fact itself. In QM a change in circumstances, such as the opening of a slit, or the addition of a force-field or a measuring apparatus will change the distribution of probability-values over possible states of an object-context pair. Yet this distribution, unlike 'probability' in the theory of observational error, but like mass, position, and energy in CPM, is not affected by shifts in the state of our cognition (Bunge (1967) and Feyerabend (1968)).

Accordingly, questions such as 'What is the probability that σ in context $\bar{\sigma}$ will appear in ΔV at t_0?' are sound in QM but unsound in CPM or STR. Conversely, 'What is the position of σ?' in QM, even if context $\bar{\sigma}$ is specified, may or may not be sound, depending on whether the superimposed amplitudes $\sum_i c_i \Phi_i$ determine a probability-density approaching unity in the very small volume dV.

4 BY WAY OF CONCLUSION, I submit that Kuhn's "normal science" is closely approximated by research directed by problems or questions that are sound under the presupposition of one or another set of axioms governing the proper syntax of a given theory. I say 'closely approximated' because Kuhn's normal science is intended to describe actual scientific practice whereas the axiomatic systems I discuss are intended only as idealisations. In practice the full proper range of syntactic presuppositions and their relation to current problems in a given field are rarely if ever explicit.

Nevertheless, Kuhn's classification of the various activities under normal science can be clarified and corrected via my idealised model as follows:

(i) The "class of facts that the paradigm has shown to be particularly revealing of the nature of things" (Kuhn (1962), p. 25) is simply the class of properties or relations mentioned in the theory's vocabulary, as governed by its proper syntax.

(ii) "Attempts to increase the accuracy and scope with which facts like these are known" (Kuhn (1962), p. 25) are endeavours to diminish the scope of error in answers to specific what-questions permitted in a particular proper syntax.

(iii) "A second but usual but smaller class of factual determinations is directed to those facts that, though often without much intrinsic interest, can be compared directly with predications from the paradigm theory" (Kuhn (1962), p. 26). These determinations are the answering of whether-questions permitted under the relevant syntax where the questions arise from deductive consequences of a particular theory formulated within this framework.

(iv) "A third class of experiments and observations ... consists of empirical work undertaken to articulate the paradigm theory." These efforts, according to Kuhn, include attempts to evaluate universal constants, the formulation of quantitative laws, and the extension of the paradigm to cases that are entirely new or related to cases for which the paradigm has already been successful (Kuhn (1962), pp. 27 ff.). These activities in my model are attempts (a) to discover or verify empirical functional relations between values for a particular physical domain of various quantities mentioned in the presupposed proper syntax and (b) to show that a given proper syntax is applicable to physical situations to which it has not been previously applied.

(v) There also occur in normal science "manipulations of the theory undertaken ... because they can be confronted directly with experiment. Their purpose is to display a new application of the paradigm or to increase the precision of an application that has already been made" (Kuhn (1962), p. 30). This activity includes the use of lawlike hypotheses formulated within the proper syntactic framework for answering various whether- and why-questions also formulated within that framework. It might also include work in pure mathematics, e.g. developing techniques for solving differential equations that express laws within the framework. This aspect of normal science might also contain the activity of conceiving different but closely related syntactic frameworks. For example, the Bernoullis, Euler and D'Alembert developed a descriptive language (a kinematics) using scalar, vector and tensor fields for the study of the behaviour of fluids. The proper syntax for fluid mechanics is similar to but not identical to that for CPM.

(vi) "Some of the problems in both the more quantitative and more qualitative science aim simply at clarification by reformulation" (Kuhn (1962), p. 32). This aspect of Kuhn's normal science, and perhaps also the conceiving of different but related proper syntactic systems, appears to lie on the borderline between normal and extraordinary science. The clarity gained in new formulations of the presupposed syntactic system may include the elimination or the addition of problems.

The discovery of X-rays includes at least three distinct achievements: The first is Röntgen's having noticed the effects of the radiation on the barium platino-cyanide screen and the second is his awareness that these effects were something unknown to physics at the time. The third achievement is the proper classification of the rays as transverse electromagnetic vibrations, an achievement to which Röntgen contributed but was not fully responsible (Whittaker (1960), pp. 348–66). This third achievement is the extension of the descriptive apparatus of Maxwell's electrodynamics to phenomena to which it had not been previously applied, a case of task (b) as described in (iv) above. The descriptive languages of classical mechanics and electrodynamics and the then current classification of radiation as electrical currents and etherial vibrations were changed only in the extent of their application, not in their internal structure.

In many cases attempts to make the proper syntax of a theory explicit may generate genuine disagreement among the members of a scientific community (Kuhn (1962), pp. 43 f.). However, wherever this disagreement occurs there is also disagreement regarding the class of problems demanding attention in the field. Nevertheless, in some cases, notably CPM, STR and (to a lesser degree) QM, the explicit formulation of the proper syntax should not elicit a great deal of genuine disagreement. In these same cases scientific research is characterised by general agreement on the content of the associated problem field.

Yet this agreement is subject to change. It may dissolve and remain dissolute indefinitely if no agreeable proper syntax is forthcoming. Or, as in the case if macro- and microphysical mechanics, agreement may dissolve regarding one and reform on another set of postulates and problems.

There may also occur changes in the shared beliefs of the community that have little or no effect on the associated problem-field or the normal scientific activity shared by that community. For example, in the eighteenth and nineteenth centuries competing views regarding the nature and the elasticity of gases shared the basic kinematic assumptions of CPM. One view assumed that gases consist of microscopic particles that interact with repulsive forces varying inversely with the distance between the particles.[1] In the nineteenth century this view was abandoned in favour of the view that gases consist of microscopic particles in random motion and whose interactions are negligible. The elasticity of gases in the latter theory is construed as the momentum of its constituent particles.

Both views shared the assumption that the behaviour of gases should be described in terms of the motions and interactions of classical material particles, and these descriptions satisfy the postulates for CPM's proper syntax. But they differed in assumptions regarding specific particular values attached to the mechanical characteristics of the particles: One view assumed the particles are motionless but exert forces on one another. The other assumed that they are always in rapid random motion but have vanishing forces of interaction.

The shift from the former to the latter of these views is a shift in the way in which a specific problem is to be solved, namely the problem of explaining mechanically the elasticity of a gas. The problem itself does not differ in the two cases. Elasticity in both is conceived as a tendency of particulate material objects to occupy an ever larger region of space. In both cases it is legitimate to ask for the number, position, motion, etc., of the gas's components, and consequently explanations are legitimately demanded of the values taken by these quantities. Hence, the shift from the first to the second of these views is hardly revolutionary in that it is not a transition from one to another normal scientific tradition.

In sum, I have attempted to show how in a given branch of science, e.g. mechanics, the problem-field and hence the direction of normal scientific research in that branch is determined by the accepted proper syntax. Some changes in accepted theory have little or no effect on the accepted proper syntax and hence also little or no effect on current normal scientific activity. The degree to which a revision of accepted theory is revolutionary is determined by the extent to which the descriptive language, the proper syntax, of that theory is altered in this revision.

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[1] This view was held by Newton and the adherents of the caloric theory of heat.

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Is the Official Theory of Mind Absurd?

by DAVID BLOOR

1 INTRODUCTION

The possibility has recently been discussed (e.g. Fodor (1968), p. 93) that the everyday and technical language of psychology, our ascription of mental conduct concepts, should be understood by likening them to the process of postulating theoretical entities in science. This suggests that one way of looking at the 'official theory of mind' as outlined as a target of attack by Ryle (1949) would be to see it as an embryonic explanatory theory of behaviour. It would then seem to be a system of ideas whose status would be rather like that of, say, atomism before Dalton.

A consequence of seeing the official theory of mind in this way is that it brings with it a means of putting Ryle's arguments in a new light. They could be transposed and turned into arguments against embryonic atomist theories. It might be that these arguments can be appraised more clearly in this transposed form. This technique of transposing Rylean arguments has already been used by Fodor (1968, p. 19) to examine Ryle's objections to the para-mechanical aspects of the official theory. The more general use of this technique can also be seen as carrying out the programme sketched long ago by Hofstadter (1951) who pointed out the 'nominalism' of the *Concept of Mind* (hereafter *CM*) and observed that it seemed to extend to Ryle's view of science.

To ensure that the transposition is a faithful one which preserves both the structure and the point of Ryle's arguments, crucial passages will be taken from *CM* and by a few alterations of words, such as putting 'atom' instead of 'mind', Ryle's own sentence structures will be used as the vehicle for the anti-atomist argument. Not all of the large number of arguments in *CM* can be treated in this way, but a number of Ryle's most typical moves will be examined. This should go some way, although only some way, towards fulfilling the programme of showing that the official theory of mind is not guilty of the charge of logical absurdity which Ryle tries to fasten upon it.

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2 THE CATEGORY ERROR ARGUMENT

The most basic of Ryle's moves against the official theory is that it mistakes the true role of mental conduct concepts and places them in the wrong category. Talk employing mental conduct concepts, instead of being construed as talk about the people we see before us, is mistakenly construed, by the official theory, as talk about the states of their minds, which are mysterious and occult entities. Really, all that there is to talk about is what we see or might see before us in the public world. To think that as well as all the data open to us, there is something else there, which is not open to us, is to reify what is essentially shorthand talk about actual or possible overt behaviour.

This argument is very familiar. What is less often noted is that it is identical with a favourite nineteenth-century argument used by those who opposed atomic and mechanical theories in physics and chemistry.

Using Ryle's own words an anti-atomist might argue that atoms are not really the topic of sets of untestable categorical propositions but the topic of sets of testable hypothetical propositions (*CM*, p. 46). Further, when we characterise things by different atomic constitutions, we are not making any untestable inferences to hidden processes occurring in their secret parts which we are debarred from seeing; we are describing the way in which macro-objects interact in a predominantly public manner, (*CM*, p. 51). The anti-atomist would no doubt preface his charge that the role of the concept 'atom' had been misunderstood, by reminding us that to be able to talk sense with a concept is not necessarily to be able to talk sense about it. To allay discomfort he may add that the purpose of the argument against the atomic myth was not to deny any facts, but merely to reallocate them, (*CM*, p. 7).

The following examples show just how close this transposed version of *CM* comes to the arguments used by those of an anti-metaphysical or positivistic bent who opposed atomistic theories. Ward (1899) talking of the chemical molecule says, 'Whether they actually exist or not they, at any rate, serve like certain legal and commercial fictions, to facilitate the business of scientific description' (p. 109, Ward is not always as conciliatory as this: p. 305 finds him opting totally for the 'symbolic character of physics'). Ward uses with approval Karl Pearson's notion of theoretical terms as 'conceptual shorthand'. In one section, which will surely be significant for any attempt to trace the history of the concept of category error, Ward remarks that physics and economics have much in common. He reminds us of the role played in economics by concepts like Ricardo's 'economic man' or the 'market' as defined by Cournot. These 'are not things we expect to meet in with real life. They are abstractions which summarise experience, not concrete realities' (p. 110). The lesson for science that Ward draws from this comparison is that, for example, to assert that the mass of the oxygen atom is a certain multiple of the mass of the hydrogen atom, represents, at best, an erroneous interpretation of the meaning of certain averages or summaries of experience. The assertion about atoms rests upon assuming that the properties of the 'average atom' are the properties of each and every atom. This is a mistake that Ward likens to the expectation that Englishmen about to marry are interested exclusively in women their juniors by 2·05 years, because tables of social statistics show this to be the difference between the Englishman and his wife.

Two things to note about Ward's position are (i) that it is a half-way house because he is being conciliatory and is assuming that there might be a range of individually differing atoms even if there cannot be an 'atom' with average properties, (ii) even so, the sort of charge made, and the examples used to illustrate it, are very close relatives to the Rylean category error, especially as illustrated by the man who expects to meet the Average Taxpayer, (*CM*, p. 18).

Ward is only one of many who have argued in this fashion. The bolder version of Ward's position, akin to Mach's, is stated by Külpe (1895). He says that if correctly understood, 'atoms would then be merely figures of speech, valuable just as far as they helped to simplify one's total conception of the interconnections of natural phenomena' (p. 125). For Külpe then, the mistake is one of taking organising concepts for terms referring to entities. This is of the general form of confusing shorthand talk about things, say averages, with talk about an extra or a special thing: as such, they are both cases of Ryle's category error.

3 DISCUSSION OF THE CATEGORY ERROR ARGUMENT

Confining the argument first to its use regarding atoms, what are the underlying reasons for insisting on seeing atoms as organising concepts or as 'conceptual shorthand'? The basic reason is the 'purely hypothetical' character of atoms, taken in conjunction with certain doctrines concerning explanation and meaning.

By calling atoms 'purely hypothetical', writers such as those above mean that they can never become a datum of experience. This means that any explanation based on atoms is going to be an explanation of established facts in terms of assumptions about a realm to which we never could have access, even if it exists. Stallo (1881), who is close to Ward, Külpe and Mach over the status of atoms, asserts that 'no hypothesis can be valid which does not identify the whole or part of the phenomena (to be explained) with some other phenomena . . . previously observed' (p. 131). This plausible principle is then taken to rule out all hypotheses which substitute, as Stallo puts it, mere assumption for fact. These, we are told, merely explain the obscure by the more obscure, or often amount to no more than a disguised restatement of what is to be explained.

That this view of explanation is similar to that held by Ryle can be seen by looking at Ryle's strictures against volitions. Official theorists are committed, says Ryle, to trying to correlate known features of behaviour with certain hypothetical causes, 'Yet this correlation could . . . never be scientifically established, since the thrusts postulated were screened from scientific observation' (p. 68). So it appears that the explanation of empirical matters can never be scientific unless it postulates mechanisms which are open to 'scientific observation'.

Stallo's interpretation of this same principle to which explanations must conform is to suggest that talk of atoms should be ruled out altogether as meaningless (p. 156). If a less extreme response is made it would, perhaps, go like this: If the realist interpretation of atomism were true it would be a mystery how we would ever know what to say about atoms. Assuming, though, that we do talk meaningfully about atoms when we use the theory, then it can be concluded that this talk *cannot* be about this mysterious realm. To see statements about micro-goings-on as being just like statements about macro-goings-on must therefore be a mistake. A reinterpretation is required which would not deny the facts available but would re-allocate them.

What is wrong with this position? If an error can be found then it may be the basis of a defence of the official theory of mind.

The error in the argument results from the particular twist which Stallo and those who are with him give to the slogan that if *A* is to explain *B*, then *A* must be something in experience, or something that we can experience. There is an interpretation of this in which it is true, there is another in which it is not. If the slogan means that we must have experienced, or be able in principle to experience, atomic events themselves before we can appeal to them in explanation, then it would indeed rule out atomic theory as it is usually interpreted; but this is too strict a demand. It is too strict because it is based on too narrow a theory of meaning. It depends on what has been called a 'direct experience' theory of meaning, in which words get their meaning by being associated with certain empirical situations, and in which they can only legitimately be used as labels for these situations. These unnecessary and unrealistic restrictions can be relaxed and replaced by what may be called a 'displaced experience' theory of meaning. The slogan of Stallo's then becomes more plausible, and it can be seen why it is attractive, but it ceases to rule out atomism. Such a theory of meaning has been worked out for scientific terms by Hesse (1966).

Hesse, along with other recent writers, takes great care to separate the conditions for the meaningful use of a concept from the conditions for its correct (i.e. truthful) application. Hesse's account of the meaningful use of theoretical concepts in science takes rise from the following rule which seems quite general: All predicates which apply to theoretical entities also apply to publicly observable entities. For example, 'charged' and 'particle' which apply to electrons also apply to pith balls and grains of sand respectively. Hesse sums up her position by saying that no predicate is in principle unobservable in *all* domains of entities. The domain of observable situations provides the rules of usage which give a concept meaning. Once launched however, its use is not restricted to this set of generating situations, but can be extended to situations which never could be used to generate the concept. To deny this possibility is to opt for a form of verificationism for which the area of meaningful use must correspond to the area in which the *truth* of the application of the predicate can be ensured. But in fact observable situations can act as models for the unobservable domains. Crudely stated, the position is that if concept A is to be used to explain event B, then although the As in question do not have to be experienceable, examples of As have to be available. The As must be something previously observed, as Stallo demands, but the experience does not have to be 'direct', that is, of *these* As; the concept can be displaced from its original context.

Given this very liberal version of empiricism, then it can be seen how the problem of appeal to hidden realms can be overcome. As long as publicly definable words are used the characteristics of hidden regions can be talked about. The denizens of this region will then indeed be the topic of sets of categorical propositions which will not need to be translated or reduced to statements about observable entities.

If this account provides a satisfactory reply to the anti-atomist argument, then because of the closeness of structure with Ryle's argument, it should work for his attack on the official theory, provided we look upon it as correctly grouped with inferred-entity theories.

Ryle objects both to the total lack of public access to minds (officially conceived) and also to the carrying over of mechanical categories from physics. The use of mechanical metaphors Ryle diagnoses as part of the origin of the category error of treating minds as special, but mysterious, entities. These two facets of Ryle's argument were, of course, proposed as mutually reinforcing. In the light of the theory of meaning sketched above, they can now be shown to be mutually destructive.

If lack of public access to a postulated entity can be swallowed in physics there is no reason why it cannot be swallowed in psychology, as long as in both cases two conditions are satisfied. First, we must know when it is appropriate to attribute a certain state to the hidden entity, and this requires public cues and signs (though we do not have to know if the attribution is true). Secondly, we must be able to say something meaningful about the hidden realm, so public rules of usage are required for the concepts employed. Some of these, for psychological terms, as Ryle implicitly acknowledges, are provided for the official theory by analogy with (public) mechanical processes, pushes, pulls, tensions, etc. What Ryle does not realise in his exposition of the official theory is that his spelling out of the process of metaphorical displacement is making good the apparent difficulties caused by lack of public access to minds. The latter poses a *prima facie* problem about how we might meaningfully talk about minds, about how we know what to say about them; the metaphorical displacement answers this problem.

So far, a general response has been made to the claim that a category error has been made. Ryle tries to substantiate the general charge by a demonstration of the absurdities which must be shown to follow in the wake of the main error. One part of the *reductio ad absurdum* mode of proof adopted is to try to show that the official picture is broken backed (*CM*, p. 20). Indeed, for Ryle it is an historical curiosity that this point was not seen before.

4 THE 'BROKEN BACKED' ARGUMENT

If the official theory were true, says Ryle, then it would be self-defeating. If what distinguishes, say, rational from irrational discourse was some state of mind, then since such a state is not open to public inspection we could never tell if our application of mental conduct concepts to other people was correct or not.

What would this argument look like if used by the anti-atomist? The argument on page 15 of *CM* turns out as follows: As a necessary corollary of atomism there is implicitly prescribed a special way of construing our ordinary concepts of substance and property. The nouns and adjectives with which in ordinary life we describe the properties and nature of the matter with which we have to deal, are required to be construed as signifying special configurations of their minute, secret parts. When something is described as soft, hard or brittle, these adjectives are meant to denote the occurrence of specific modifications of their atoms. No one has access to these atomic events to tell if they are correctly or incorrectly applied. The potter, the metal worker, the glass blower, could thus never assure himself that his comments had any vestige of truth. Yet it was just because we do in fact all know how to make such comments about the properties of things, make them with general correctness and correct them when they turn out to be mistaken, that natural philosophers found it necessary to construct their theories about the nature and properties of things. Finding descriptions of substances and their properties being regularly and effectively used they sought to fix their logical geography. But the logical geography officially recommended would entail that there could be no regular and effective use of these concepts.

As an attack on atomism this argument is based on a total misunderstanding. Talk about atoms is meant to be an explanation of the features of the manifest world. Atomic events are not meant to provide criteria for the application of empirical descriptions. Atomic states do not compete with manifest features of things as the basis for calling this brittle, or that hard. It is true and important that what were once purely descriptive labels may begin to be taken as carrying implications regarding atomic states; but this does not mean that the theory requires impossible access to the atomic constitution of things before descriptive terms can be applied.

Exactly the same sort of reply can be given to Ryle. The demand that we take an impossible peep into another's mind before we can call someone clever need be no part of the official theory. We label someone clever because he tends to know all the answers. We may then try to explain this or account for it by assertions about what goes on in his mind. This is exactly the same as calling an object brittle because it shatters, and then trying to explain this fact by making categorical assertions about its hidden structure. The explanation in either case *may* be feeble, uninformative and untestable. None of these failings, however, makes the procedure absurdly broken backed.

5 PRIVILEGED ACCESS AND COMPETING JUDGMENTS

So far the similarity with atomism has shown how to answer the basic charge as spelled out by Ryle. There is, though, an asymmetry between the atomic and official theories that needs to be examined. The asymmetry is brought about by the privileged access to mental states that is a feature of the official theory of mind. It may be argued that this feature has implications which are sufficient to destroy the claim that the official theory is to be treated like an embryonic scientific theory with its own special theoretical entity. The objection goes like this: If a theory generates propositions that conflict with empirically determinable states of affairs then the theory is false. On the official theory one way in which propositions about the relevant theoretical entity, the mind, may be generated is by the owner of that mind making assertions. Indeed, the notion of privileged access requires that such assertions cannot be overruled. But, by reverting to the analogy with scientific theories, the usual way in which states of minds are attributed to people, is on the basis of behavioural cues. These two sources may produce conflicting judgments, as when Jones insists that he is not jealous, but his behaviour shows that he is. The privileged access premise of the official theory seems to demand that theory can override empirical judgments in a way that is not paralleled by scientific theories, even embryonic ones.

Despite appearances to the contrary the points outlined in the objection do not destroy the parallel with scientific theory, and further, the problem of competing judgments is one that the official theory can cope with perfectly well. To begin with, the objection depends upon too simple a view of the way in which theories and facts confront one another. Duhemian sophistication provides the beginning of an answer. A predicted atomic state of affairs, say, may not be accompanied by the empirical cues that one would expect. This does not mean that the atomic state does not obtain, provided one can think up plausible reasons why, in the circumstances, the atomic state might be accompanied by the empirical cues that are observed. In science it is just a matter of fact that empirical states of affairs do not constitute criteria (in the strong Wittgensteinian sense) for hidden states. Failed predictions do not entail falsity, only inadequacy, and the normal scientific response is to complicate the theory, not drop it. Too rapid, too *ad hoc*, and too massive a resort to such elaboration of a theory obviously diminishes the respect that it can command, but it does not make it guilty of logical absurdity.

If privileged access by Jones leads him to say that he is not jealous, but observation leads others to assert that he is, then there is a *prima facie* conflict, but it is one that Duhemian moves within the official theory can reconcile. The moves may be at the expense of simplicity and perhaps at the expense of whatever systematic power the theory previously had (this remains to be seen) but they will not necessarily be at the expense of logical consistency. The epicycle that is required to resolve the *prima facie* conflict can be phrased in more than one way. The common-sense way is to distinguish between the state that a person feels he is in and the state that he *is* in. If Jones, whom we trust, sincerely assures us that he harbours no resentment then we must find a way of viewing the matter so that he is not guilty of lying. What we accept is that it is true that Jones has no *feelings* of jealousy. We have to reconcile ourselves to the fact that people can be jealous without feeling it. We could preserve something closer to the unsophisticated version of the official theory by keeping it as a 'law' in the system that if Jones did not *feel* jealous then Jones *was* not jealous, and then argue that the jealous action followed from a malignant agency other than Jones, which acted through him, perhaps taking possession of him. Possession theories are now unfashionable, but they point to one way out of the present difficulty and give testimony to the strength of the urge to equate the self with the introspectible self. The more technical approach would be to introduce the notion of, say, unconscious jealousy. The notion of an unconscious want or desire is introduced, and learnt, to cope with precisely the cases in which the insistence of a person about his wants or desires is taken seriously, but has to be reconciled with conflicting behavioural cues. Privileged access as such is not denied or repudiated by the modified form of the theory, although the scope and significance of what is open to it is modified.

In case it should be automatically assumed that such moves weaken the theory, it is worth remembering that the notion of the unconscious has had considerable power in the hands of thinkers working within the terms of the official theory; not only Freud, but Helmholtz in his work on perception, has utilised this notion. This complicating move, outlined above, certainly signifies the departure of the official theory from its Cartesian origins in a number of respects, but on Ryle's own (rather grudging) admission the notion of the unconscious is without doubt a part of at least one variant of the official theory (*CM*, p. 14).

6 THE VACUITY AND INFINITE REGRESS ARGUMENTS

One of Ryle's most frequently used arguments is the charge that the official theory is either vacuous, or generates infinite regresses. The error according to Ryle is that the official theory tries to explain certain actions or performances or utterances by appeal to prior mental actions or mental performances or mental utterances, volitions or cognitive acts. Ryle uses these arguments to try to establish logical flaws in the official analyses of the relation between knowing how and knowing that (p. 30), the self-luminous nature of consciousness (p. 163), the relation between plans and actions (p. 176), and in many other cases.

Certain of these arguments can be transposed to form anti-atomist arguments. Because the attack is used in such a variety of cases by Ryle the transposition will not necessarily work in all cases. For example, the use of the argument against the self-luminous nature of consciousness seems to require special treatment (Locke, 1968), but the transposition is appropriate for the other cases mentioned.

When looked at as an argument against the use of atomic theories the charges of vacuity and threatened infinite regress turn into exactly the arguments used by Stallo, (1881, Chs. 7 and 8). In considering the relation between the atomic theory and chemical laws such as the law of definite proportions, Stallo says (p. 126) that the atomic theory:

> accounts for them as it professed to account for the indestructability and impenetrability of matter, by simply iterating the observed fact in the form of an hypothesis. It is another case (to borrow a scholastic phrase) of illustrating *idem per idem.* It says, the large masses combine in definitely-proportionate weights because the small masses, the atoms of which they are multiples, are of definitely-proportionate weights. It pulverises the fact and thereby claims to have sublimated it into a theory.

This quotation has been offered at length to show that the vacuity charge can be presented with considerable rhetorical force. How is the charge to be answered?

7 DISCUSSION OF THE VACUITY AND REGRESS CHARGE

The answer to the vacuity charge is that it simply does not matter. Even though it looks like a serious criticism it establishes, in fact, no *logical* flaw in a theory at all. It is an objection based on methodological not logical grounds. If 'vacuous' is taken, by stipulative definition, to mean that there is a one-one correspondence of theoretical to empirical terms, then indeed the arguments above show the theory to be 'vacuous'. Normally, however, to establish that a procedure is vacuous implies that it is pointless and barren and is not worth pursuing. It is precisely these overtones of the usual word 'vacuous' that are *not* established by a mere demonstration of a one-one correspondence of the sort mentioned. To think that to establish the narrower point at the same time establishes the broader point is to forget that vague ideas can be made precise, that crude theories can be refined, that what starts off as merely a way of talking can turn into something much more.

This can all be clearly seen in the case of the atomic theory. The one-one correspondence that Stallo argues for is certainly the mark of a theory which cannot predict and which is as complex as the data that it is supposed to explain. Think, though, of the consequences if Stallo's methodological suggestion had been followed and the theory discarded, if the stipulated sense of 'vacuous' had been broadened into the ordinary sense. The existing atomic theory would not have been able to grow out of the older version. It would have had, most improbably, to spring forth complete and entire, with no developmental links to older, 'vacuous', untestable versions.

On these grounds it is suggested that since there is no logical flaw in having a theory as complex as the thing to be explained (though to rest content with it is to be deplored methodologically) the one-one correspondence argument should not be accepted as contributing to Ryle's proposed demonstration of absurdity, nor should it be accepted as a reason for discarding the official theory of mind any more than it was so accepted in the case of atomic theory.

8 OBJECTIONS TO THIS DEFENCE OF THE OFFICIAL THEORY

It might be said that the defence rests on psychological grounds. Just because later theories grow out of earlier ones, it does not follow that the earlier ones were free from conceptual confusion. The critic of atomistic theories who is concerned with logical propriety can acknowledge that sometimes good consequences flow from belief in the logically absurd, he would merely insist that good consequences do not prove logical soundness and cannot therefore be brought in evidence. The argument of the previous section, however, is not that because the earlier theory was useful, therefore it was logically sound (and perhaps even psychologically necessary, for later theories). It was that, given as argued above that it was logically sound, the sort of 'vacuity' demonstrated was not an adequate reason for discarding it, and that, indeed, this would have been a bad strategy.

If all of this is applied to the official theory then it can be entirely accepted that the theory as it stands has no explanatory power. What cannot be accepted is that it is absurd or should be discarded. The parallel with atomism might however be further challenged by remarking that although the old untestable atomism was capable of generating really scientific theories, the official theory seems doomed to sterility; which would surely be presumptive evidence for the Rylean diagnosis of radical logical confusion. This objection would be reinforced by the multitude of Rylean arguments which apparently play havoc with the terminology of the official theory. Volitions are postulated, so, demands Ryle, how many volitions did you execute before breakfast, were they quick and easy or slow and hard? No one knows how to answer these questions.

The beginning of the reply to this group of objections is that the lack of similar refinement of the official theory is not such an established fact as it may appear. Notice that it is not being argued that the existence of refinements proves the logical soundness of the official theory. Logical soundness has been argued for independently by trying to meet Rylean objections. The argument here is merely that the ground for suspicion created by the alleged lack of refinements is not as strong as it may first appear.

An example of a relatively modern refinement has already been mentioned in the case of the Freudian systematisation of the notion of the unconscious mind. A field, though, in which much rapid refinement is going on is the one where from a Rylean point of view it would be particularly unwelcome, namely the field of linguistic behaviour. Ryle (*CM*, pp. 295–6) derides the idea that when a person wittingly uses a phrase or sentence 'there must antecedently or concomitantly occur inside him a momentary something, sometimes called 'the thought that corresponds with the word, phrase or sentence'. These are ghostly doubles which do nothing to account for the significant use of speech. When speaking we do not, says Ryle, do two things, say something aloud after a shadowy performance.

Ryle is surely right to stress how little has been said by merely postulating a shadowy thought. But the fact is that theories have been developed which attempt a causal account of the intelligent use of speech. These theories contain reference to covert, momentary, antecedent events or states which could correspond to the thought expressed by the utterance. Theories have been attempted, then, which refuse to be contained within the very restrictive methodological demands which follow from Ryle's theory. Ryle would permit experimental psychology to explain why things go wrong but not why they go smoothly. Psychologists are charged with the task of explaining why slips of the tongue are made, but forbidden to develop an overall picture of what is going on when slips are not made (*CM*, p. 326). It is doubtful if this is a coherent methodological demand. An account of why something fails to work, without an implied account of how it does work normally, is suspiciously like a grin without a cat. Even if it is coherent, there is no reason why psychologists should obey this injunction when physicists, chemists and engineers do not. No one tries to develop science of metal fatigue in abstraction from an understanding of the structure of unfatigued metals. For the psychologist, speculation on the details of 'shadow' processes seems impossible to avoid if he is going to explain, as distinct from merely note, such common or garden phenomena as the difference between a sentence uttered by a person and the same sentence uttered by a parrot. That there is a difference (though not necessarily in sound), the behavioural and linguistic context of the utterance makes clear: but this is not yet to try to explain the cause of the difference. This may be a question of no interest if one is professionally concerned only to elucidate the more immediately obvious criteria for distinguishing between the performances in the first place: but to go beyond this demands postulating a mechanism, or para-mechanism, which begins to account for the differences in linguistic competence of the two speakers.

How will the shadow process as postulated, say, by theories in psycholinguistics evade the charge of vacuous repetition, that they merely redescribe the original data? The answer is, that as far as they do escape this charge, it will be because they unify diverse data. For example, theories have been proposed which lean heavily on the idea that linguistic behaviour requires the embodiment of a 'grammar', which is, crudely, an axiomatic system for generating the sentences of a language. Such grammars were held to be an integral part of the mechanism responsible for linguistic performances such as speaking or recognising sentences. As Fodor and Garrett (1966) put it in their review of this work:

> the hypothesis that grammatical operations . . . are psychologically real, turns in large part upon whether there exists a correspondence between such formal features of the derivation of a sentence as length and such performance characteristics of the sentence as perceptual complexity (p. 143).

Initially, as the review shows, such evidence was forthcoming, but recently the trend has been reversed and the extreme theoretical simplicity of the model has been widely questioned. But though the theory is almost certainly wrong, and rests upon moves which are by no means necessary inferences (i.e. 'a grammar formalises the speaker's linguistic information . . . therefore (it is) a component of whatever system of mechanisms is involved in the production of speech' (Fodor and Garrett (1966), p. 139), this does not make it absurd. Not only does the theory unify a range of empirical data about different sorts of performances, recognition speed, difficulty of recall of sentences, etc., it also makes contact with data and theories about puzzling features of language learning.

Philosophers would insist, of course, that the mere existence of a theory, even if it unifies data, does not prove that it is not conceptually confused. They would ask: If the intelligent performance of such serial tasks as speaking is to be explained by the performance of a shadow serial task, does not *this* have to be performed intelligently too? Is there not an infinite regress here? The standard reply to this seems perfectly adequate. This consists in saying that whilst overt performances can properly be called intelligent, the covert para-mechanical performances which cause the overt behaviour are to be appraised in an appropriate para-mechanical appraisal vocabulary. Does the para-mechanism work quickly or slowly, continuously or intermittently, etc.?

This reply now invites objections based on Ryle's claim that we have nothing to say in reply to even the most obvious questions about para-mechanisms such as volitions. With regard to the official theory this cannot be denied, but it can be plausibly put down to its lack of refinement. After all, there was a time when thinkers had no well motivated answers to offer to simply formulated questions like, how big are atoms?, how are their weights related?, etc. As soon as the mechanisms or para-mechanisms are stated in more detail then similar questions can be formulated and sensible answers produced. For example, just as Dalton's atomic theory had to start off with a bold but rather arbitrary decision about the chemical formulae of certain compounds (his rule of greatest simplicity) so a theory of the mind might have to begin by stipulations to the effect, say, that one volition produces one response in a reaction time experiment. In this case then at least the time to execute a volition could be measured. If difficulty of a volition is held to be related to the minimum time to execute it then difficulty could be related to the number of alternatives from which the response had to be selected. As might be expected from the history of other disciplines, such questions as Ryle poses need to be related, at least to begin with, to adequately simple situations. This indeed was what was beginning to happen to the official theory, as Myers' (1911) textbook of experimental psychology shows.

Suppose that it was accepted (though this would be quite counter to Ryle's position) that it is appropriate to try to formulate a systematic causal account of behaviour; then it might be held that nothing said above about shadow process should give comfort to the official theory, because the only legitimate form that such an account could take would be as a theory of brain processes.

This position may have two forms. In the first form it is an article of faith backed up by an (optimistic) extrapolation of successes in physiology. In reply it must be insisted that it is, after all, a contingent matter whether all the structures, for which the psychological theorist requires embodiment, can be provided by the physiologist. It is as contingent a matter as whether the metallurgist can always provide materials which will satisfy the requirements, in terms of strength, flexibility, hardness etc. that the engineer may demand if his designs are to be realised. Should such physiological embodiment not be forthcoming, then postulation of 'embodiment' in a non-material substratum is an expedient which (if the arguments so far are sound) is not absurd. As Chomsky (1968) has pointed out, this was precisely the relation that obtained in the seventeenth century between the known specifications of behaviour and the known capabilities of physiologically comprehensible systems.

The other form of this position has been outlined by Fodor (1968, pp. 97–9). Fodor argues that believing in the inferred entity account of the mental demands that some form of materialism be adopted; the most plausible form being one which equated mental events with neurological events. This position, as developed by Fodor is, interestingly, dependent on exactly the same premises as led Ryle to his position and Stallo to anti-atomism. It rests upon the premise that 'claims about inferences to *T* (the inferred entity) can only be true where talk about observations of *T* make sense' (p. 98). Although Fodor explicitly disavows an empiricist approach to science, a form of verificationalism seems to be lurking in his position here. Fodor gives a twist to his premise (that it makes no sense to say that something is a necessarily private entity), identical to the twist that Stallo gave his premise mentioned earlier. The force of the claim that nothing can be a necessarily theoretical entity can be allowed for by recognising, as argued above, that though all concepts must have a public application for some range of entities it is not necessarily the one in which they are currently being used. If this is true, it means observable states do not have to be conjured up to count as observations of the theoretical entity in question, as when brain states are proposed as candidates for mental states. A liberalised version of empiricism thus shows that Fodor's inference from a theoretical entity view of mind to materialism is not a necessary one.

If these two replies are sound then a systematic psychological theory can legitimately be developed in the form of a theory of the conscious and unconscious mind, because if systematic psychological theories are possible at all they may *have* to take this form. To consider building theories of mind however, brings with it the need to discuss the final Rylean argument that will be considered here; the alleged absurdity of mind-body interaction.

9 THE INTERACTION ARGUMENT

Ryle's form of the argument, against the possibility of a coherent conception of the relation of minds to bodies, is based on the fact that the 'actual transactions between episodes of the private history and those of the public history remain mysterious since by definition they can belong to neither series'(p 12).

To transpose this argument brings out how curious it is. It would be as if the anti-atomist were to argue against atomic theory on the grounds that the 'actual transactions' between the micro and the macro were suspect because they belonged neither to the story about purely atomic goings-on, nor to the story about purely macro-events.

The answer to this, which can be applied directly to Ryle's form of the argument, is that the problem of the status of the actual transactions is quite easily coped with by means of bridging principles. Bridging principles are propositions of the sort which assert that, say, the pressure of a gas in a container (macro-level) is caused by the momentum from the impact of tiny particles on the side of the container (micro-events). Of course these bridging principles do not contain only terms with an empirical reference, nor only terms with a reference to theoretical entities. Such principles are not (wholly) in the observation language, nor (wholly) in the theoretical language. This truism does not, as Ryle seems to think, preclude their existence, it is merely a condition that they must satisfy; to wit, that they relate terms of both kinds. Nor is the possibility of such principles precluded by the fact that there is no public access to the theoretical entities in question. Bridging principles can be, and usually are, postulated rather than found by observing 'correlations'.

It is concluded that Ryle's argument, as it stands, goes no way towards establishing any difficulty with regard to mind-body interaction.

10 CONCLUSION

No account has been taken of other arguments against the official theory of mind than those offered by Ryle, for example those stemming from Wittgenstein. This is partly because it seems to the author that such arguments have already been refuted (Locke, 1968). In any case, arguments or clusters of arguments have to be met one at a time.

The point of this paper has been to try to show that some of Ryle's arguments do not succeed in contributing towards his programme of showing that the official theory of mind is absurd. This is because, it is claimed, they are based on nineteenth-century empiricist assumptions of scientific method, meaning and explanation. It has been a central feature of the defence that the official theory be treated as an embryonic explanatory theory, with the mind as an hypothetical entity. This practice is in accord with other recent writers and with the approach of experimental psychology. The method of argument has depended on the move (previously used by Fodor) of transposing Ryle's arguments into another area where their shortcomings can, perhaps, be more clearly seen. In this case parity of reasoning was established with certain nineteenth-century objections to atomism. In no part of the argument has it been assumed that the official theory is true. The defence has been strictly against the charge that the official theory is logically absurd.

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Euclidean Space: A Lasting Philosophical Obsession

by T. G. McGONIGLE

1 IS PHYSICAL THREE-SPACE Euclidean? The question is ill-formulated. According to relativity theory, the basic entity we have to work with is space-time; following the general theory of relativity, I shall assume that space-time is a four-dimensional, differentiable, Lorentz signature manifold. If space-time satisfies certain conditions (see Anderson (1967) Chapter 10) we can project out a three-space (a three-dimensional space-like manifold with a positive definitive metric); and in general there are many ways to do the projection. In the general theory of relativity one encounters physically interesting space-times for which the space of one projection is flat while the space of another projection is curved; it can even happen that the space of one projection is infinite while the space of another projection is finite (i.e. compact) (see Schrödinger (1956) Chapter 1). Thus, the above question must be relativised to a given projection.

We do have evidence that space is non-Euclidean in the following sense: for cosmological models satisfying Einstein's field equations and agreeing with observational data, certain natural projections yield spaces which are curved and/or finite.

2 Lucas (1969) has attempted to rehabilitate the Kantian doctrine that space is necessarily Euclidean. Lucas' arguments are designed to support an interpretation of this doctrine in which space is taken to be "neither the pure mathematician's construct nor the remote object of the sophisticated physicist's discovery, but the space of our ordinary experience, given to us, and given to us under a standard interpretation, in all our visual and tactile experience" (p. 2). It is not clear to me exactly what sense of space Lucas has in mind. By 'the space of our ordinary experience' he does not mean the space of our visual experiences since as he himself points out this space is non-Euclidean (see pp. 6–7). The space Lucas is concerned with is the space relevant to describing the real shapes of objects (see p. 7); but is not this the physicist's space? Perhaps Lucas would give an affirmative answer, but would reply that he is concerned only with those aspects of the physicist's space which can be ascertained in ordinary experience. But what does ordinary experience consist of?

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For example, does the use of scientific instruments like telescopes fall within the domain of ordinary experience? Lucas does not say. Moreover, even if we limit the ordinary man to naked eye observations made with the help of a cheap rule, it is still easy to think of physically realistic situations in which such a man, brute that he may be, can yet convince himself of the non-Euclidean nature of space—this is especially so in case the global topological structure of space is non-Euclidean (see Putnam (1962) for relevant examples).

Since I am unsure what Lucas' doctrine is, I cannot be sure that I can refute it. On the other hand, it can be shown that most of Lucas' arguments are unsound. I now turn to this task.

3 AFTER OFFERING a simplicity argument for Pythagoras' rule for combining measures along different directions, Lucas adds that "Mathematicians investigating differential geometries, which are not Euclidean, take care to posit nonetheless that they are 'Locally Euclidean,' that is to say that

$$ds^2 = dx_1^2 + dx_2^2 + \ldots + dx_n^2$$

The concession, though indeed on a small scale, could hardly be larger" (p. 9). Now any Riemannian space is infinitesimally Euclidean (Lucas says locally Euclidean, but a Riemannian space may fail to be locally Euclidean in the sense that it may not be the case that every point is contained in an open neighbourhood which is Euclidean), but modern differential geometry treats spaces which are more general than Riemannian spaces; for example, Finsler spaces may not be infinitesimally Euclidean (see Rund, 1959). As we shall see below, there is, at least *prima facie*, some reason for thinking that physical three-space must be Riemannian, and hence, infinitesimally Euclidean. But I fail to see how one can conclude with Lucas that this concession to Euclideanism though on a small scale "could hardly be larger." The concession is an important one, but it is hardly large enough for Lucas' purposes since it does not extend to finite regions of space, and ordinary experience—whatever that is—certainly concerns finite objects.

4 LUCAS' MOST POWERFUL sounding argument concerns the concept of rigidity. We are told that Euclidean Geometry "emerges as the geometry in which the three operators of *displacement*, *rotation*, and *reflection* are possible without alteration of geometrical properties" (p. 5). Now "Measuring rods may or may not be rigid in the event; but there would be no sense in even thinking of using them unless we thought within a geometry in which there could be objects whose geometrical properties were unchanged by rotation and displacement" (p. 8). Therefore, Euclidean

Geometry provides the only ecological environment in which rigidity is a viable concept (p. 8).

This argument is patently unsound. To begin with, the second premise does not mention reflections, and this omission seems proper—the concept of rigidity does not seem to be concerned with reflections but only with translations and rotations. This difficulty might be overcome by claiming that we want to work with oriented spaces, and hence, that we want to consider operations which preserve the orientation. This would rule out reflections, but whether physical three-space or the space of our ordinary experience is in fact or must be orientable seems to me open to question. However, I shall not press this point since more important ones need to be made.

Let me begin the discussion of rotations and translations by assuming that physical three-space S is a three-dimensional, differentiable manifold equipped with a positive definite metric. Consider first rotations. To make the discussion more definite, let us assume that S is a Finsler space so that the distance ds between points $P(x^i)$ and $Q(x^i+dx^i)$ can be written in the form $ds = F(x^i, dx^i)$. Since the distance between distinct points must be positive, we require that F is non-negative and vanishes only if all the dx^i vanish simultaneously. F is also required to be linear homogeneous to the first degree in the dx^i for positive factors; that is, $F(x^i, k\, dx^i) = kF(x^i, dx^i)$, $k>0$. This condition on F is necessary and sufficient for the distance

$$\int_{t_1}^{t_2} F\left(x^i, \frac{dx^i}{dt}\right) dt$$

along a curve $x^i = x^i(t)$ to be independent of the choice of parameter t. Riemannain spaces are a special case in which F takes the form $(g_{ij}(x^k)\, dx^i dx^j)^{\frac{1}{2}}$, where the g_{ij}, the components of the metric tensor, are independent of the dx^i.

The question now becomes, which Finsler spaces allow for the rigid rotation of objects? Helmholtz was able to show that all and only Riemannian spaces allow for the rigid rotation of sufficiently small objects. More precisely, all and only Riemannian spaces have the property that for any point $P \in S$ and for any two directions in the tangent space T_P at P, there exists an isometry of T_P onto itself which takes the first direction into the second (see Laugwitz (1965), p. 183).

It might be objected that the discussion so far has little to do with ordinary experience which, as we admitted above, concerns finite objects. This point is well taken, and it does lead to a very special class of Riemannian geometries; for if the possibility of the rigid rotation of objects of any finite size is required, then the space must be of constant curvature (see Laugwitz (1965), p. 184).

Having arrived at Riemannian spaces of constant curvature, we can now ask which of these spaces allow for the possibility of the rigid translation of finite objects. The answer is that every such space does; conversely, only those Riemannian spaces of constant curvature allow for this possibility. The relevant technical theorem is as follows. When and only when a Riemannian space S is of constant curvature does the following condition holds: for any two points P, $Q \epsilon S$ and any two orthonormal triads of vectors at P and Q, there exists a neighbourhood of P that can be mapped isometrically into a neighbourhood of Q in such a say that P is mapped into Q and the triad at P is mapped into the triad at Q (see Laugwitz (1965), pp. 141–2).

Thus, Lucas' argument fails. Any Riemannian space of constant curvature allows for the possibility of rigid rotations and translations.

Where does Lucas go astray? It is true, as Lucas points out, that in Klein's *Erlanger Program* Euclidean Geometry emerges as the geometry in which displacements, rotations, and reflections are possible without alteration of geometric properties. On one reading, this truth is nothing more than a truism—in the *Erlanger Program* a geometrical property in the Euclidean sense is defined to be a property which is invariant under the group of Euclidean transformations. More precisely, a geometry is defined in terms of a space S as a group G of transformations on S. A property of subsets of S is said to be a G-property if it is the case that whenever $s \subseteq S$ possesses the property, then so does the image of s under G. The geometry which (S, G) defines is then the study of G-properties. In this context Euclidean Geometry is seen as the geometry defined by taking S to be Euclidean space and G the Euclidean group.

Doubtlessly Lucas had a different reading in mind according to which geometrical property means a property needed to describe objects in the space of ordinary experience. On this reading the above statement does have content, but (even assuming it to be a true statement) it still does not constitute the basis of an argument of the sort Lucas wants because of the presence of the qualification *in* Klien's *Erlanger Program*. As a means of defining a geometry, the *Erlanger Program* was too narrowly conceived; for modern differential geometry, e.g. the study of the differential geometry of metric spaces, cannot be subsumed under Klein's scheme in any non-artificial way (see Cartan, 1927). Moreover, the sense which the words 'Euclidean Geometry' take on in the context of Klein's *Program* is not a sense relevant to Lucas' purposes. What Lucas needs to show is that *Euclidean space* (or Euclidean geometry in the sense of the study of, say, the differential geometry of Euclidean space) is the only space (geometry in this sense) in which rigid translations and rotations can take place; and we have already seen why Lucas cannot establish this —it is simply not true.

5 IN ADDITION to the unsoundness of Lucas' claims about rigidity, there is a gap between these claims and the conclusion that the space of ordinary experience must be Euclidean. Lucas proposes to fill this gap by arguing that Euclidean Geometry is "a condition of our experience as passive spectators, as active—or at least mobile—spectators, and as agents" (p. 8). The part about active-mobile spectators is supposed to follow from the claims about rigidity discussed above and the assertion that "If we did not pick out properties that were invariant under reflection, rotation and displacement, we should be unable to recognize as the same what we see in a mirror and what we see when we look direct, what we see from one side and what we see from the other, and what we see from afar off and what we see from nearby" (p. 6). But the word 'properties' in this assertion can refer either to features of the observed object itself, e.g. its size and shape, or the way the object appears to the observer. I know of no geometry or physical theory according to which properties in the first sense change as the observer changes his perspective, and every optical theory I am familiar with predicts that the properties in the second sense will change as the observer changes his perspective. We recognise as the same what we see from one side and what we see from another because, for example, we know how the appearances of objects change as we change our perspective.

The part about passive spectators is supposed to follow from Lucas' claims about rigidity and the assertion that "if we did not pick out properties that were invariant under rotation and displacement, we could not form the concept of a material object, something we can push around without affecting its properties" (p. 6). This is an interesting assertion, but it stands in need of defence. Most material objects we ordinarily come in contact with can be pushed around without drastically altering their size and shape, but *prima facie*, this fact does not seem to provide an essential element of our concept of material object—things like spatio-temporal continuity seem much more important.

6 LUCAS HOLDS that it is "a necessary condition of our being able to apply the concept 'same shape though different in size' that our geometry should be Euclidean" (pp. 3–4). From the preceding sections we see that Lucas

must show that Euclidean space is the only space in which the concept of similar but unequal figures applies. And here there are known results which make it seem to me quite possible that Lucas' claim can be justified (though not in the way indicated in Lucas' essay) if the conditions necessary for applying the concept are made strong enough, e.g. if it is required that "given any figure, another figure is possible which is similar to the given one and of any size whatever" (Lucas, p. 3). For instance, it is known that Euclidean space is the only geodesically complete (i.e. any geodesic can be indefinitely extended) Riemannian space which admits a proper similarity mapping (i.e. a similarity mapping which is not an isometry) onto itself (see Laugwitz (1965), pp. 159–60). An argument similar to the one used to establish this result can also be used to show that the following condition on S forces S to be flat, though possibly S may have a non-Euclidean topology: for any $P \epsilon S$ and any spherical neighbourhood N of P, there exists a spherical subneighbourhood N^1 of N containing P and a similarity mapping of N onto N^1 which leaves P fixed. But I fail to see how we get from (what I am willing to take to be) the fact that Euclidean space is the only Riemannian space which allows for a sufficiently rich concept of similarity of geometric figures to the conclusion that the space of ordinary experience must necessarily be Euclidean. Is it a necessary truth (or even a truth) about the space of ordinary experience that for any given figure, there is another similar to the given one and of any size whatever? I do not see that it is (but perhaps this is because I do not understand what the space of ordinary experience is), nor does Lucas offer any argument on this point.

7 I CONCLUDE THAT Lucas has not succeeded in rehabilitating the Kantian doctrine that space is necessarily Euclidean; and I believe that the above discussion strongly suggests that any such rehabilitation must fail. I also believe that the discussion provides evidence for what I take to be a sad fact: although philosophy, especially in the last fifty years, has contributed a great deal to our understanding of the foundations of many fields of knowledge, it has contributed very little to our understanding of the nature of space and time and the nature of spatial and temporal experiences.[1]

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[1] I wish to thank Charles Bogle for several helpful suggestions.

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Discussions

CANFIELD'S FUNCTIONAL TRANSLATION SCHEMA

IN AN ESSAY on functional explanation in biology, John Canfield (1964) developed a translation schema that renders functional statements clearly nonmentalistic and therefore acceptable in biology. The schema runs (p. 292):

> A function of *I* (in *S*) is to do *C* *means* *I* does *C*; and if, *ceteris paribus*, *C* were not done in an *S*, then the probability of that *S* surviving or having descendants would be smaller than the probability of an *S* in which *C* is done surviving or having descendants.

The *ceteris paribus* clause is to be understood according to the following (p. 291):

> The *ceteris paribus* clause assumes that the two specimens, e.g. the two mice, are: (i) alike in other relevant respects (i.e. respects other than *C*), and (ii) in other respects normal. The *ceteris paribus* clause also assumes that the two specimens are in environments which are: (iii) in relevant respects the same in both cases, and (iv) normal.

Hugh S. Lehman (1965) posed as a counter-example the functional statement

> A function of the heart (in human beings) is to produce a pulse.

In his reply, Mr Canfield (1965) argued that persons lacking a pulse would not have poorer chances for survival or begetting because the *only* things missing would be (*a*) the pulse and (*b*) whatever the pulse produces. Two action-sentence chains describing part of what the heart does are (p. 330):

> (1) The heart circulates the blood.
> The circulation of the blood carries nourishment to the cells.

and

> (2) The heart produces a pulse.
> The pulse results in (such and such).

According to Mr Canfield, removing (2) would leave (1) intact, for "the heart, in achieving the transportation of nourishment to the cells, does not do so via the production of a pulse (though of course in achieving transportation a pulse is, as a matter of fact, produced)" (p. 330). If (1) remains intact and the cells continue to be nourished, what need is there for a pulse? "Suppose we could add to an animal's heart an instrument which resulted only in making the animal's blood flow at a steady rate, rather than in spurts. So far as we know this change would neither raise nor lower the animal's chances of survival" (p. 330).

Although the pulse does not produce the circulation and although the circulation does not produce the pulse, one and the same thing produces both, viz. the beating heart. The pulse cannot be removed (unless something is added, as a pulse dampener) without removing the cause of it, the beating heart, any more than can the light be removed from a burning match that is producing both heat and light (unless something is added, as a shade) without removing the burning match. Mr Canfield does, of course, recognise the one-many relation of things and what things do, for he said, "though of course in achieving transportation a pulse is, as a matter of fact, produced" (p. 330). But he does not draw the implication that in order to remove one of the effects without bringing in other mechanisms, the cause must be removed. Nor does he realise that to bring in other mechanisms (such as his pulse dampener) involves him in violating his own *ceteris paribus* clause.

Mr Canfield says: "Now to suppose that S_1 is like S_2 except that in S_2 C is not done is to suppose that S_1 is characterised by all true sentence chains, whereas S_2 is characterised only by a subset of the true sentence chains" (p. 330). But, if my above analysis is correct, the characterisation of S_2 would not be a subset of the characterisation of S_1, for, along with the absence of action-sentence chains containing C, description of S_2 either would have new action-sentences describing the added mechanism or would lack action-sentences containing I (the heart) and what it produces, including, in the example above, circulation and the transportation of nutrients to the cells. That is, if S_2 has an added mechanism, then along with

(1) The heart circulates the blood.
The circulation of the blood carries nourishment to the cells.

would be added

(3) The heart pumps blood to the pulse dampener.
The pulse dampener removes pressure variations in the blood.

If S_2 does not have an added mechanism, action-sentence chain (1) would be omitted. But if circulation ceases when the pulse ceases, then the probability of survival and having descendants is lowered. This would, according to Mr Canfield's formula, render pulse a function of the heart. (A parallel argument could be constructed rendering heart sounds a function of the heart.)

On the other hand, if S_2 can differ from S_1 not only in the absence of C but also in added mechanisms, then not only does the translation schema (correctly) not allow producing a pulse to be a function of the heart, but it also (incorrectly) does not allow cooling the skin to be a function of perspiration. If a pulse dampener is allowable in the one case, a plastic skin covering and modification of the lungs should be allowable in the other. The description of the new S_1 would include

Perspiration cools the skin.
Cooling the skin contributes to temperature homeostasis.

The description of S_2 would omit the statement about cooling the skin but would add statements about the plastic cover and panting.

Perspiration moistens the plastic layer.
The plastic layer prevents cooling of the skin.

and

Panting cools the lungs.
Cooling the lungs contributes to temperature homeostasis.

But, assuming the panting to be as efficient as perspiring, there seems to be no survival advantage in perspiration. If Mr Canfield, in the pulse example, could say that since circulation and transportation of nutrients to the cells is maintained, nothing else matters and therefore the pulse is not a function of the heart, so we can say, in this example, that since the contribution to temperature homeostasis is maintained, nothing else matters and therefore cooling the skin is not a function of perspiration. (The objection that human beings do not, as a matter of fact, normally pant is answered by the reply that neither do they have pulse dampeners.)

The example of the bottle capping machine (Canfield (1964) p. 292) differs from the pulse and perspiration examples in that the screeching wheel presumably is not linked to the rest of the machine in such a way that silencing the wheel would require permanently removing or damaging a needed part. If we modify the example so that it has such a linkage, then silencing the screeching wheel would make the machine inoperative and we should conclude that a function of the wheel is to screech.

As an example which has such a linkage naturally and, in addition, no added mechanisms, consider

> The heart circulates the blood.
> The heart uses energy.

Using energy surely fits Mr Canfield's notion of a 'doing' (1964, p. 288; 1965, pp. 328-9). It is something that the heart does. The heart also circulates the blood. But it could not circulate the blood without using energy. So we can say that if the heart did not use energy, the probability of survival and having descendants would be lower than if it did use energy. Hence, a function of the heart is to use energy.

An especially interesting example for any prospective translation schema is one which has two or more completely redundant systems built in. Although common in electronic hardware, complete redundancy seems rare in organisms. At best, examples are rather strained. Ignoring that, what would be a translation schema for this example?

> A function of an ambidextrous person's right hand is to grasp objects.

I am not suggesting that there is something misguided about the attempt to formulate criteria for functional statements in biology. When left at the informal stage, it seems perfectly clear that we can talk about whether or not an organ or a process or a habit or whatever contributes to or is useful for the survival of that organism and its species. But there seems to be unacknowledged difficulties in formally unpacking 'contributes to' and 'useful'.

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KUHN AND THE COPERNICAN REVOLUTION

THOMAS S. KUHN states that Copernicus' system is neither simpler nor more accurate (observationally) than the Ptolemaic system (Kuhn, 1962,[1] p. 169; 1962,[2] p. 75). If this is so, and if simplicity and observational accuracy are the only rational grounds for preferring one scientific theory to another, then those people who preferred Copernicus' system to Ptolemy's must have done so from non-rational motivations. Kuhn agrees with this and suggests one such possible motivation in the following passages.

> as Copernicus himself recognized, the real appeal of sun-centered astronomy was aesthetic rather than pragmatic. To astronomers the initial choice between Copernicus' system and Ptolemy's could only be a matter of taste, and matters of taste are the most difficult of all to define or debate. (Kuhn, 1962,[1] p. 171)
>
> There must . . . be a basis, though it need be neither rational nor ultimately correct, for faith in the particular candidate [theory] chosen. Something must make at least a few scientists feel that the new proposal is on the right track, and sometimes it is only personal and inarticulate aesthetic considerations that can do that. Men have been converted by them at times when most of the articulable technical arguments pointed the other way. When first introduced, neither Copernicus' astromical theory nor De Broglie's theory of matter had many other significant grounds of appeal.' (Kuhn, 1962,[2] p. 157)

I do not wish to deny the efficacy of such arguably non-rational factors as aesthetic appeal in causing people to prefer the Copernican system. However, it can be shown that, contrary to what Kuhn says, the Copernican system is also less arbitrary than the Ptolemaic in several clear-cut ways, so that, other things being equal, the Copernican system was preferable on undisputedly rational grounds. (Of course, other things presumably weren't equal since Ptolemy's system fits in somewhat better with the general Aristotelian framework, for which there was not yet an adequate alternative.) I shall mention three planetary phenomena for which the explanation on Copernicus' view is clearly simpler than on Ptolemy's.

1 THE MAXIMUM ELONGATION of Venus and Mercury. The inferior planets, Mercury and Venus, share an observational characteristic which differentiates them from the superior planets. It is that they are always seen relatively close to the sun. Mercury's maximum elongation varies from 18° to 28° and Venus' is about 47°. In the Ptolemaic system with the earth at the centre and the planets and sun going around it, this can only be accounted for by postulating a certain relationship between the motions of the inferior planets and that of the sun. In particular, it is assumed that the centres of the epicycle of Venus and Mercury are always in line with the sun. In the Ptolemaic system this is simply an arbitrary assumption required by the facts but not explainable by any deeper principles of the theory. In the Copernican system, that there is a maximum elongation for Venus and Mercury follows immediately from the fact that Venus and Mercury are inside the Earth, and this ordering of the planets follows in turn from a deep seated principle common to both Ptolemy and Copernicus, namely the longer a planet's period of revolution, the greater its distance from the centre.

2 RETROGRADE MOTION and opposition. The superior planets only retrogress when they are in the opposite part of the heavens from the sun. In the Ptolemaic system retrograde motion is determined by the planet's motion along the epicycle while position in the heavens is primarily determined by motion along the deferrent. An arbitrary relationship, therefore, must be postulated between these motions and also between these motions and that of the sun. In Copernicus' theory a superior planet appears to retrogress when the Earth overtakes it on the inside. The planet, therefore, must be on the outside, that is, away from the sun. Thus again a relationship which is arbitrary for Ptolemy—necessitated by the facts but not a consequence of anything else in the theory—is a natural consequence of the basic Copernican viewpoint.

3 FREQUENCY OF RETROGRESSIONS. Of the superior planets, Saturn retrogresses most frequently, Jupiter next, and Mars least frequently. With the inferior planets, Mercury retrogresses more frequently than Venus. In the Ptolemaic system the frequency of a planet's retrogression is determined primarily by the frequency of its revolution around its epicycle. However, that Saturn should revolve in its epicycle more rapidly than Jupiter or Mars is not implied by any fundamental principles of the theory; rather it is again an arbitrary fact dictated by the observations. For Copernicus, retrogression of a superior planet occurs when the Earth overtakes that planet and retrogression of an inferior planet occurs when that planet ovetakes the Earth. Since the Earth overtakes Saturn most frequently, Jupiter next, and Mars least often; and since Mercury overtakes the Earth more frequently than Venus, the facts about frequency of retrogression are immediately accounted for.

Several further instances quite analogous to those above could be pointed out and indeed were pointed out by Copernicus. But perhaps the three mentioned will be sufficient to demonstrate my point that many features of the Ptolemaic system are quite arbitrary from the standpoint of theory. In so far as Copernicus' system has fewer of these arbitrary features, it seems to me obviously preferable on rational grounds.

Kuhn is well aware of the planetary phenomena I have mentioned and seems to recognise that the Copernican system is in some sense superior in the way it accounts for them. But he suggests that this superiority is not rational. He calls it aesthetic, a 'personal and inarticulate aesthetic consideration', a 'matter of taste'; and he seems to infer from this that it wasn't rational (see the second passage quoted above). But Kuhn makes no attempt to show either that these considerations are 'merely' aesthetic or that aesthetic considerations are not rational. In any case, they are obviously neither personal nor inarticulable, and I see no grounds to dismiss them as non-rational. This example does not, therefore, as Kuhn supposes, lend rational support to Kuhn's hypothesis of the non-rationality of scientific change.

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A CONDITIONAL VINDICATION OF THE STRAIGHT RULE

THE STRAIGHT RULE is one of a set of inductive rules which, from evidence concerning a sample, makes a prediction concerning the population from which the sample is drawn. This paper puts forward a vindication of the rule in that, of the set of inductive rules, only the straight rule satisfies a necessary condition of success.

For the purposes of this discussion a set of objects will be characterised by an ordered couple $\langle n, \psi_n \rangle$: the first term denoting the cardinal number of the set, the second the property with which the inductive inference is concerned. In this terminology an inductive rule will assert conditionally, given a sample $\langle n, \psi_n \rangle$, a suitable value ϕ_n with which to characterise the population of size N, $\langle N, \phi_n \rangle$. The straight rule is that inductive rule for which $\phi_n = \psi_n$.

Most philosophical discussions of induction have been concerned with infinite or quasi-infinite populations (e.g. von Mises (1939), Braithwaite (1953), Reichenbach (1949), for instance the drawing, with replacement, of counters from a barrel. What reason is there to suppose that scientific practice is concerned only with inferences to infinite populations? It is a matter of controversy in astronomy (Hoyle, 1955) whether the universe has infinite extension and whether it contains an infinite number of objects. The biological, geological and social sciences are even less obviously confined to infinite domains of investigation.

It has been argued (Hempel, 1965) that a characteristic of a scientific *law* is that it should be unrestricted spatiotemporally in its possible application. That the domain of application of the law is potentially infinite does not entail that it is actually infinite. In a few years the laws relating to the contagion and effects of smallpox may have no application, but this in no way reduces their truth which is independent of the number, finite or otherwise, of their instances.

Reichenbach (1938, p. 261 f.) has attempted to vindicate the straight rule by reference to a convergence condition: if a sample exhibits a property ψ, then the sequence of values of the property of samples of increasing size must have as its limit the value of the population property, the population being infinite. Consequently any inductive rule which does not, in the limit as the sample size approaches infinity, assert of the population property what is true of the sample, must make a false assertion concerning the population property. Of all inductive rules only those which are convergent can, in the long run, correctly predict the population property. The straight rule, being one of the set of convergent rules, is thereby vindicated.

The situation can be symbolised as follows: let a sample of size n be characterised by $\langle \mathrm{n}, \psi_n \rangle$. Let the infinite population be characterised by $\langle \infty, \theta \rangle$. The convergence condition for a randomly chosen sample (or pool of samples) is then

$$\lim_{n \to \infty} \langle n, \psi_n \rangle = \langle \infty, \theta \rangle \qquad (1)$$

Consider now an inductive rule R which may be characterised as a function having sample descriptions as arguments and population descriptions as values. $R\langle n, \psi_n \rangle = \langle \infty, \phi \rangle$ where ϕ is a prediction of a population property. Note that the function R does not predict the population size since this is given.

Since the population property is *ex hypothesi* unknown, (1) is not provable. However a necessary condition of the truth of (1) is

$$\lim_{n\to\infty} \langle n, \psi_n \rangle = \lim_{n\to\infty} \langle n, \phi_n \rangle \tag{2}$$

That is, the rule must, in the limit as $n \to \infty$, assert of the population what is true of the sample. The condition (2) is a syntactic property of a rule and, if true, may be proved. Since the straight rule satisfies $\psi_n = \phi_n$ for all n, it satisfies (2), and hence (1), and is thus proved to be convergent.

So far it has been assumed, as by Reichenbach, that the population is infinite. However if, as was argued earlier for some scientific examples, the population is finite, the convergence condition may be strengthened. We will now require that when the sample size reaches the (finite) population size, the value or degree of the predicted population property must equal the sample property for reasons exactly parallel to those given by Reichenbach. To show a rule to be convergent we must have

$$\psi_n = \phi_N \text{ when } n = N. \tag{3}$$

When the value of N is known, no constraint is placed on the form of the inductive rule, since the equation (3) is compatible with any (physically possible) value of ϕ_N for $n \neq N$.

When the value of N is not known two alternatives arise. Either ϕ_n is taken to be a function of N or it is not. If ϕ_n is taken to be a function of N then we are in the dubious position of making inductive rules depend upon unknown empirical magnitudes. And if N is guessed, and guessed wrongly, then the convergence condition must be violated.

If ϕ_n is not taken as a function of N then neither can it be a function of n. For if it were a function of n and not of N the convergence condition (3) could not be proved. The only rule that satisfies the convergence condition and is not a function of n is the straight rule.

The only rule for inducing population properties from sample properties, in the case of actually or possibly finite populations of unknown magnitude, which is convergent and whose success does not depend upon correct estimates of the population size is the straight rule.

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CONFIRMATION AS A PROBABILITY: DEAD BUT IT WON'T LIE DOWN!

1 Miss Baillie has recently (Baillie, 1969) meticulously demolished a successful old argument of Sir Karl Popper's. Popper's argument (1959, Appendix* ix) was directed successfully against Professor Carnap's early attempt (1950) to explicate *Grad der Bewährung* as a probability: Carnap admitted as much and improved his explication to avoid the contradiction Popper pointed out (Carnap, 1962). Miss Baillie shows impeccably how Carnap's improved treatment is not vulnerable to Popper's old argument. Nevertheless, the conclusion she drew, that "we can comfortably hang on to both the direct interpretation and the Relevance Criterion" (p. 50), is not warranted by her arguments, and, indeed, would be difficult to support: for this is how Miss Baillie states what she says we can comfortably hang on to:

(1) y confirms x if and only if $p(x,y) > p(x)$;
(2) degree of confirmation of x on $y = r$ if and only if $P(x,y) = r$ (p. 46).

In this note I argue for the rejection of both (1) and (2).

2 Clearly, (1) is useless as a general criterion of relevance, for, as Popper pointed out in the starred appendix to which Miss Baillie makes detailed reference, when x is a general hypothesis $p(x) = 0 = p(x,y)$ from which it would follow that no general hypothesis could ever be confirmed. Popper repaired the criterion by adding $p(y,x) > p(y)$, thus (p. 359):

(1′) y confirms x if and only if $p(x,y) > p(x)$ or $p(y,x) > p(y)$.

It was the second disjunct that Professor Mackie found, as a criterion of relevance, in many varied attempts to resolve the paradox of the ravens (Mackie, 1963). Relativised and in his own symbols, his Inverse Principle (as he called it) was:

(1″) b confirms h in relation to k if and only if $p(b,k.h) > p(b,k)$

It was not Miss Baillie's (1) but Mackie's (1″) that Professors Hooker and Stove renamed the Relevance Criterion of Confirmation (Hooker and Stove, 1967). Whether (1″) may be hung on to comfortably is another question: (1) should surely be rejected.

3 And with Miss Baillie's Relevance Criterion should go also her direct interpretation. Of course, Miss Baillie is at liberty to retain the name 'degree of confirmation' for any measure she chooses—though some uses of the name are more confusing than others, as it appears Carnap discovered and regretted. What none of us should hang on to is the direct interpretation of what scientists and philosophers of science want to call confirmation as a probability—especially if we wish to retain a relevance criterion which picks out as especially relevant those instances which come to light as a result of a genuine test of an hypothesis. Let us suppose that Professor Watkins (1960) more or less adequately captured the central notion of a test with his view:

b is a genuine test of h if $p(b,k.h) > p(b,k)$

Then we should perhaps want to make degree of confirmation proportional to the difference between $p(b,k.h)$ and $p(b,k)$—rather than proportional to $p(h,b)$. Just such an interpretation of confirmation characterises many recent attempts to formalise degree of confirmation (or corroboration or factual support). (See Kyburg, 1964, p. 257.)

Recta interpretatio requiescat in pace.

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Reviews

ACHINSTEIN, Peter (1968) *Concepts of Science: A Philosophical Analysis*. Baltimore: Johns Hopkins Press. n.p. Pp. xvi+266.

This is an important book. Essentially its objective is to examine the actual nature of definitions, terms, theories and models in science, paying due regard to the variety of the ways in which reputable scientists have expressed themselves and the diversity of what current text-books take to be established scientific knowledge. Patiently and systematically Professor Achinstein shows in turn that from this point of view there is some error or oversimplification in each of the different accounts of scientific theory-construction that positivist philosophers, like Carnap or Hempel, have already proposed or might think of proposing. Then he does the same for other philosophers, like Feyerabend, who have been influenced by the history of science to claim that the meaning of every term we use depends on the theoretical context in which it occurs, so that if one theory replaces another no term that occurs in both can have the same meaning in both. Even the hallowed distinction between observational and theoretical terms turns out to be a broken reed. 'Certain terms can be grouped together', writes Achinstein, 'and attention to relevant similarities may prove instructive; but in each such case, any criterion placing terms into one of two categories will apply only to certain terms, not to all terms used in science or even to all those in a particular theory.' Similarly he argues that, 'if in seeking a theory of models and analogies one means to be looking for some specific set of characteristics shared by all and only models and analogies reference to which will provide considerable illumination for these devices, then . . . one is looking in vain'. In particular the semantical theory of models, which holds that essentially a model for a scientific theory is an alternative interpretation of the calculus underlying that theory, is incapable of dealing satisfactorily with the richness and complexity of the facts.

Perhaps the best way to indicate Achinstein's method of argument is to summarise, very briefly, his list of, and distinctions between, the different kinds of analogy or model that are found in natural science.

First, he lists seven examples of analogy, such as the analogy drawn by Huygens between waves of light, water and sound or the analogy drawn by Maxwell between the electric field and an imaginary incompressible fluid flowing through tubes of variable section. He points out that the similarities crucial to the analogy may be of several different kinds. For example, they may be constituted by satisfaction of the same physical principles or by sameness of geometrical configuration. But the scientist who points to such analogies always intends thereby to help provide a better understanding of something by referring to its analogue. In Maxwell's day hydrodynamic phenomena were better understood than electrical ones; hence Maxwell thought it enlightening to compare the latter with the former.

Secondly, Achinstein distinguishes four different types of representational model. In 'true' models all those characteristics of the prototype that are important to the scientists or engineers concerned are reproduced to a set scale with respect to given quantities: e.g. a model of a bridge is constructed in which length, width, and thickness are reduced by a factor of 100. In 'adequate' models only some of these characteristics are reproduced: e.g. a model of a bridge is constructed in which deflection is made to be a function of the moment of inertia of the cross-section rather than of the width and depth. In 'distorted' models different scales are used: e.g. a model of a bridge is constructed in which the width is reduced by a factor of 100 and the length by a factor of 50. In 'analogue' models the characteristics of the prototype are just represented, rather than actually reproduced: e.g. an electrical circuit is frequently treated as a model for an acoustical system. But in all four cases the model is intended to represent the prototype in such a way that facts about the prototype can be ascertained by examining, making calculations with respect to, and possibly experimenting upon, the model.

Thirdly, there are theoretical models such as the billiard ball model of a gas or the Crick-Watson model of the DNA molecule. Theoretical models share four or five main characteristics though they may differ in others. A theoretical model is a set of assumptions about some type of object or system. These assumptions describe a type of object or system by attributing to it what might be called an inner structure, composition or mechanism, reference to which is intended to explain various properties exhibited by that object or system. A theoretical model is treated as a simplified approximation that is useful only for certain purposes. It is proposed within the broader framework of some more basic theory or theories (e.g. the billiard ball model within the framework of Newtonian theory). And a theoretical model is often formulated, developed, and even named, on the basis of an analogy between the object or system described in the model and some different object or system. Where both a theory and a theoretical model are available, explanation and systematisation via the theory are often said to be deeper and to provide more insight. But a theoretical model may afford calculations that are impossible or exceedingly difficult with the theory; it may advance our understanding of the theory and even afford insights that lead to its extension or modification; and it may serve an important didactic role.

Fourthly, Achinstein distinguishes what he calls 'imaginary' models, such as Poincaré's model of a Lobachevskian non-Euclidean world, with a temperature decreasing towards the circumference, all bodies having the same coefficient of dilation, and so on. Again, the object or system is described by a certain set of assumptions. But the proponent of the model does not commit himself to the truth, or even the plausibility, of these. The point of an imaginary model is to show what the object or system could be like if it were to satisfy certain initially specified conditions. Thus it can demonstrate that certain suppositions, which might otherwise be thought self-contradictory or inconceivable, are at least consistent, and even that their actual consequences might be worth investigating. Such a model can also promote understanding of the principles it exemplifies.

In short Achinstein's attitude to science is rather like that which Wittgenstein in his later years adopted towards language. Every theory, or well-nigh every theory, has its own structure. Science is all right as it is, and philosophers have no business to generalise about it in a way that could only be made true by altering some parts of science so as to fit the generalisation. The task of the philosopher of science is to record, describe and compare, not to reconstruct or reform.

Now this is an interesting and important point of view, which tends to have been represented in the literature hitherto only by popular expositions and other relatively superficial treatments. Achinstein has set new standards of thoroughness, insight and understanding for such an approach. Intrinsically, however, it is an approach that cannot be criticised by its own standards, except on points of historical detail. The only general point that can be made against it is that it would be a very great pity if every philosopher adopted it. For, just as what Achinstein calls a theoretical model has a certain value that justifies its association with a scientific theory, so too (and in the same three respects) a revisionary or reconstructionist philosophy of science may have a certain value that counterbalances the inevitably oversimplified picture it presents if regarded as a description of the actual facts. Such a philosophy may allow calculations (about degree of evidential support, e.g., or explanatory power) that are exceedingly difficult in text-book science; it may advance our understanding of textbook science and even afford insights that lead to its extension or modification; and it may serve an important didactic role.

Perhaps one should add, too, that the cool, detached presentation of similarities and differences that Wittgenstein advocated in regard to language, and Achinstein advocates in regard to science, is an ideal to which it is very difficult to conform all the time. Even Wittgenstein is sometimes accused, for example, of retaining traces of a verificationist metaphysic within the overall framework of his philosophy of language. Achinstein is perhaps open to a similar accusation in regard to his own elucidation of the concept of a theory. He chooses to approach this elucidation through considering the question: what does it mean to say that someone has a theory? He then lays down six conditions in terms of which he proposes to characterise scientific theories. To say that some T is a theory is, depending on the context, to say one or more of the following: that (at least) some person now satisfies the six conditions with respect to T; or that some person at a previous time satisfied these conditions with respect to T; or that it is plausible to imagine some person as having satisfied these conditions with respect to T; and so forth. On the face of it this point of view is in flat opposition to that expressly advocated by Popper in certain recent papers (in particular in his 'Epistemology without a Knowing Subject', *Logic, Methodology and Philosophy of Sciences*, III, ed. van Rootselaar and Staal, 1968, pp. 333 ff.), where he claims the existence of a largely autonomous third world of objective knowledge, objective problems, and objective theories. So it is curious that Achinstein does not regard his proposals here as being at all controversial. There seems rather to be a deep and difficult problem about how to choose, or what is at stake in choosing, between Popper's realism and Achinstein's conceptualism. But if we adhered to the philosophical methodology that Achinstein elsewhere advocates it might well turn out that no such choice was necessary. Perhaps scientists themselves talk both about Newton's mechanics, say, or Bohr's model of the atom, and also about electro-magnetic theory or valency theory *tout court*.

Accordingly either Achinstein would have done better to claim that both realist and conceptualist notions of a theory exist side by side in actual science or he needs to justify his own apparent preference for conceptualism by arguments that, whatever their other merits, can scarcely accord with his express methodology.

Despite these minor criticisms, however, I am quite sure that Achinstein's is a book which no one can afford to miss who is at all interested in problems about the nature of theory-construction in the sciences. It expresses an important point of view much better than it has ever been expressed before in the philosophy of science.

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NIDDITCH, P. H., Ed. (1968) *The Philosophy of Science.* Oxford University Press. 10*s*. Pp. 184.

This book is a useful, varied and inexpensive collection of papers which have already been published elsewhere, none of them before 1958. It is not easy to appraise philosophical compilations because usually no single point of view emerges clearly enough to become the focus of review. Mr Nidditch explains that these papers have common concern with scientific explanation, but even so one reads the book as a collection of individual items. The editor warns that it is no good looking for a single structure for explanation; science is too varied for that. This is true, but it makes one regret the more that nothing on the philosophy of biology or the social sciences is included. The book is intended for the undergraduate and the general reader. An undergraduate would certainly benefit from reading it if he already had some philosophy, but the really general reader would find it hard going. Now for some of the contributions individually.

Mr Feyerabend pleads passionately and persuasively for 'tolerance in matters epistemological'. He thinks that empirical knowledge on any matter grows only by considering as many alternative theories as possible so that the significance of the commonest observation is repeatedly called into question. And so there can be no such thing as 'meaning invariance', whereby the meaning of some terms is context-independent. And he is right. But this does not require what he also seems to demand, that at a given moment a given term has only one meaning, its current scientific meaning. It may have more than one meaning, each appropriate to a different context and each dependent upon movements in that context. What the term 'object' means to the scientist is different from what it means to the man in the street, although there is sufficient analogy for us to use the same term in both cases.

The following article by Mr Schlesinger deals with Carnap's theory of empirical meaningfulness and its shortcomings. In this theory the empirical meaningfulness of a term M depends upon the coherence with existing knowledge T of a statement involving M, S_M, which entails a decidable observation statement S_O. This coherence means essentially that S_O must be predictable from $T+S_M$. But what is it to make a prediction from $T+S_M$, when we have not yet defined empirical meaningfulness for an O term? Do we have to define this, and if not, why do we have to do it for M's? It seems to me that to make meaningful predictions we must assume empirical meaningfulness for at least some terms—we cannot establish this by making predictions. This is a shorter answer to Carnap than those offered by Mr Schlesinger.

There is a paper by Professor Hempel on explanation in science and history and a helpful one by Professor Nagel on teleological explanations. Professor Hempel tries to show that historical explanations can be as law-like as scientific explanations even though they deal with human action. The argument seems to me to hinge upon whether the characteristics of a person can properly be regarded as independent empirical properties or not. Professor Nagel reassures us about teleological explanations. He points out that they are not incompatible with the more usual causal explanations. It is as legitimate to focus on ends as on beginnings for men 'in the middest of things'.

May Brodbeck in her paper 'Meaning and Action' observes that though 'my arm rising' and 'my raising my arm' may have the same referential $\text{meaning}_{(a)}$ (what goes on), they do not have the same intentional $\text{meaning}_{(b)}$ (what the act is about). She asks—can $\text{meaning}_{(b)}$ be accounted for without resorting to mental terms, so that the objectivist can talk about mind? She thinks it can, but I am not convinced. What 'X running hard for a bus' $\text{means}_{(b)}$ to an objectivist is simply that X wants to catch it, but it may $\text{mean}_{(b)}$ to X that he will be late for work and suffer from his conviction that unpunctuality is morally reprehensible if he misses it. How could an objectivist possibly know this sort of thing? Men do not, indeed cannot, always wear their hearts upon their sleeves. But there is very useful discussion here on the various senses of 'meaning' and 'understanding'.

Professor Goodman and Mr Ackermann deal with induction and simplicity, and Professor Black and Mr Achinstein discuss self-supporting inductive arguments. Black is interested in whether we can give inductive support (R′) for the use of inductive arguments (R). He seems to think we can, but Mr Achinstein is not so sure. Suppose R′ is '$x\%$ of R's have been successful so there is an $x\%$ chance that this R will be', and R is '$y\%$ of oranges have been juicy, so there is a $y\%$ chance that this orange will be'. And suppose $x = 90$, $y = 80$. Black would now argue that if R′ is significant, we must say that there is a 90% chance that this orange will be juicy. What then are we to make of our generalisation about oranges based presumably upon good evidence? I cannot see what bearing R′ could have upon R's, indeed I do not think that Black has shown how such an R′ could be set up anyway.

Mr Barker and Mr Achinstein argue with Professor Goodman on the latter's 'new riddle of induction'. This has to do with the preferred projection of non-temporal predicates. Goodman constructs two predicates, 'grue' (green if time is prior to t, blue if time is not prior to t) and 'bleen' (blue if time is prior to t, green if time is not prior to t), and argues that they are not more temporal than 'blue' or 'green' because of symmetry in their interdefinition. But Barker and Achinstein observe that if this is so, speakers of the 'grue-bleen' language must exercise a different sense of colour perception from speakers of the 'blue-green' tongue. As is pointed out, this is not logically impossible, but would it not dictate a change of physics also?

The final paper by Mr Mackie is on the 'paradox of confirmation'. His discussion and bibliography indicate clearly the attention philosophers have given to this problem, which is important because it exposes some of the general presuppositions involved in testing scientific claims.

The book concludes with a generous list of items for further reading.

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ANGELELLI, Ignacio (1967) *Studies on Gottlob Frege and Traditional Philosophy.* Dordrecht, Holland: D. Reidel. f.48. Pp. xvi+291.

The aim of this book (which is apparently the author's doctoral dissertation) is to explore the connections, similarities, and differences between Frege's philosophy and various semantical, logical and ontological doctrines in Western philosophy, especially those arising in the Aristotelian tradition. The author makes few concessions to his readers. They are expected to be able to read not only English, but also German, French, Italian, Latin and Greek. (The habit of not translating quotations into the language of the main text is, of course, fairly common, and occasionally justified, but in general it is to be deplored). They are expected to be familiar not only with the main doctrines of Frege but also with a great deal of ancient and scholastic philosophy: almost always when a text is referred to (whether in Frege or elsewhere), the discussion assumes that the reader is either familiar with it or has it open before him. This is a book to be read only in a library. Further, the book is very difficult to read since, besides requiring great resources of scholarship, it frequently proceeds in a very unsystematic way (much of the book is made up of miscellaneous remarks comparing the ideas of this or that philosopher with those of Frege) and secondly because the author has indulged in the luxury of hundreds of footnotes at the end of chapters, with no very clear principle of division between what goes into footnotes and what goes into the main text, so that in reading the book one wastes an enormous amount of time turning to footnotes without being able to tell in advance which add something significant to the text, which are remotely relevant digressions, and which are merely page or paragraph references. What a pity the four persons who revised and corrected the English text (acknowledged on p. 261) were not asked to comment on the general style and layout, for a clearer and more readable version could be very useful.

Historically-minded readers of this journal armed with a suitable library and much pateience, may well profit from joining the author's exploration of the similarities and differences between the views of Frege and other philosophers on such topics as: Ontology (e.g. the relation between Frege's object-function distinction and the traditional singular-universal and substance-accident distinctions); Predication Theory; Semantics (e.g. Frege's idea of different senses corresponding to the same object is compared with traditional discussions of different aspects of the same thing); Hierarchies of Levels (*Stufen*) of functions and predicates; Existence; Classes (e.g. could Frege in *Grundlagen* have regarded classes as agglomerations *qua* structured by a given concept?); and Number. The author concludes (p. 260) that Frege continues many other classical themes besides those connected with formal logic, 'thereby obtaining new insights in several important areas'. This much I suppose the book establishes, even though its expositions of both Fregean and non-Fregean ideas are often very unclear, and some of the comparative remarks seem to be inaccurate. To illustrate these criticisms, I shall follow the author's example and make a few 'miscellaneous remarks'.

The statement of Frege's programme on page 4 poses the problem: 'Do arithmetical propositions belong to the class of propositions which may be proved in a purely logical way, or to the class of truths which must be established by recourse to empirical facts?' However, Frege took very seriously (see *Grundlagen* sections 5 and 88, for example), the third, Kantian, alternative that they may be synthetic (i.e. not purely logical) yet *a priori*, and his rejection of this alternative (by contrast with his acceptance of Kant's views on Geometry—see *Grundlagen*, pp. 19–21, 101–2) is surely one of the two main features of his logicism, the other being the reduction of all arithmetical concepts to 'accepted logical notions'. Angelelli, however, claims (p. 251 n. 109 and p. 259) that the *only* acceptable way of understanding the significance of Frege's logicism is 'by contrast with a past philosophical tradition where numbers were not generally regarded as transcendentals'. Even the paragraph headed 'Frege's philosophical logicism' (p. 244) fails to bring out very clearly the above two main features namely (*a*) the reduction of arithmetical *proofs*, (*b*) the reduction of arithmetical *concepts*, to logic.

In section 2.25 it is suggested that since Frege had already acknowledged (e.g. *Begriffsschrift*, section 8) that the object referred to was not all that mattered in connection with a name but also the manner of presentation of the object, he should not have accepted the Leibnizian principle of substitutability *salva veritate* of names with the same referent: he would not then have had to offer his theory of indirect reference to deal with the apparent exceptions. But this suggestion shows a failure to recognise that in view of Frege's analysis of concepts as functions, the Leibnizian principle is just a special case of the general fact that a given function can only determine *one* value for a given argument: this is part of what 'function' means. Thus to abandon the principle would require a complete revision of Frege's whole theory. He might have tried to introduce a new sort of function, whose value depends not only on the argument but also on the way it is referred to (they might be called *qua*-functions, since they determine values for arguments *qua* so and so). He chose instead to economise on varieties of functions at the cost of complexity of linguistic rules. To suggest that he should have developed a different theory is not enough: we need to be told how the alternative would work out in detail, and what effects it would have on the rest of Frege's semantic theory.

On pages 54 ff. an attempt is made to explain Frege's view that the truth-values are the *bedeutungen* of sentences in terms of the ambiguity of the word 'Bedeutung'. Since the word can mean 'importance' and since truth-value is what is important about sentences we have a simple explanation of Frege's puzzling view. Or have we? Isn't the *Sinn*, the thought expressed by the sentence, equally important? Whether the ambiguity of 'bedeutung' affected Frege *subconsciously* we shall presumably never know, but had he been consciously influenced by it he would surely have said so. In any case, the *main* reason for taking truth-values as references of sentences is that it enables Frege to give a uniform analysis of all complex signs, whether referring phrases, complete simple sentences, or compound sentences, in terms of arguments and functions. Could anything other than the truth-value of a sentence remain unaltered (*a*) through all replacements of a sentence with others expressing the same thought and (*b*) through all replacements of *parts* of the sentence with new parts having the same *bedeutung* (i.e. extensionally equivalent parts)? One might at first think that the notion of 'a possible state of affairs' could do the trick, but because of (*a*) and (*b*) the boundaries of such states of affairs would be fluctuating and indeterminate, and it would be hard to prevent them expanding so that all true sentences end up denoting the whole universe: an alternative which seems no more acceptable than Frege's own. Admittedly, Frege's own argument, in 'Sinn and Bedeutung' is far from clear: perhaps he did not at the time see clearly that the best justification for his claim that sentences denote truth-values was its indispensability for a major part of his semantical system!

On page 153 there is a discussion of the passage in Frege's *Begriffsschrift* (see section 9) where he says that unless a sentence contains a quantifier the distinction into function and argument 'has nothing to do with the conceptual content; it concerns only our way of looking at it'. Angelelli claims that Frege is saying that arranging a sentence in different ways according to what is argument and what is function does not make any difference to 'What counts for deduction' except when a name in that sentence is replaced by a universally quantified variable. However, it is clear from what Frege says later (section 11) that what he is getting at is that when quantifiers are used in a sentence their contribution to the meaning (or 'conceptual content') depends on a prior analysis into function (constant part) and argument (replaceable part). The content of 'Plato admires Socrates' does not require any particular subdivision into functions and arguments (at least three alternative possibilities are obvious) whereas 'Everyone admires Socrates' can *only* be understood in terms of the results of substituting different names in the function-sign '. . . admires Socrates': so the occurrence of the latter, unlike '. . . admires . . .' or 'everyone admires . . .' as a function-sign in the complete sentence is essential for its conceptual content (e.g. essential for what determines its truth or falsity). This is why in Frege's symbolic notation a *variable* is required to represent the structure of 'Everyone admires Socrates', but not to represent the structure of 'Plato admires Socrates'.

Another extreme illustration of Angelelli's inability to state Frege's views clearly is found on page 161 when we are told Frege asks his readers to think of 'entities given under quite different guises: a number may be hidden under Julius Caesar or a geometrical direction under England'. This is a reference to Frege's claim (*Grundlagen*, pp. 68 and 78) that to define equivalence relations such as 'same number', 'same direction' is not yet to identify *the* number, or *the* direction associated with any given class or line, or to say what *things* are numbers or directions.

Despite these faults, there are many interesting points, and the book should provide a useful bibliographical guide for anyone interested in its comparative programme. It would be somewhat more useful if the short subject index (with a mere thirty-four entries) were enlarged. Curiously, although the book is written (mostly) in English, the bibliography of Frege's main works makes no reference to any English translation.

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JÁNOSKA, Georg (1962). *Die Sprachlichen Grundlagen der Philosophie.* Graz, Austria: Akademische Druck- und Verlagsanstalt. n.p. Pp. 145.

This is a re-issue, without changes, of a book which first appeared in 1962, but which has so far escaped review in this journal. It is a contribution to the growing literature in German on that variety of approaches to philosophy which have come to be lumped together under the label 'linguistic'. As such one welcomes it; and one welcomes its appeal for greater clarity in epistemology, and its emphasis on the continuity between traditional and recent philosophy. But at the same time there is a regret that it has not been better done, for to the reader who received his philosophical training at an English or American university nearly all of it will appear rather superficial.

On page 21, for example, the Positivists come in for some harsh words:

> Sie nehmen die Sprache nicht so, wie sie ist, in ihrer Mannigfaltigkeit, in ihren vielfältigen Formen, in ihrem Usus, in ihrer Vieldeutigkeit, die Genauigkeit nicht ausschliesst. Sie tun so, als ob die Bedeutung eines Satzes ein für allemal feststünde, unabhängig sei von seinem Zusammenhang. Sie tun so, als ob es gleichgültig wäre, wer einen Satz, wann und zu wem er ihn spricht und in welcher Betonung. Sie verkennen den Unterschied von geschriebener und gesprochener Sprache.

This is easy enough to say. It would be harder to state just which tenets of Positivism rely on these errors crucially, rather than merely verbally. Were the Vienna Circle really committed to the view that a philosopher can make a pronouncement about the meaningfulness of a sentence which will hold for every utterance of it, in spite of all the possible variations of context, intonation and so on? Would they not have replied that of course these features make a difference, but that there are limits to the difference they can make—in particular, they cannot make a meaningful assertion out of an utterance unless they provide it with empirical verification conditions? In this they may have been mistaken, but they were not just being dogmatic and arbitrary, as Jánoska will have it. An argument for their view is to be found, for instance, in Schlick's paper, 'Meaning and Verification'. It is based on certain assumptions about the way in which language is learnt, and these do not include any of the rather obvious mistakes which are listed in the passage I have quoted.

Another perplexing point is a sentence (p. 30) in which Carnap's distinction between Intension and Extension is compared to Jánoska's 'Bedeutung'/ 'Bezeichnung':

> Während sich nämlich die Bedeutung (in unserem Sinne) vom Wort bzw. Satz nicht trennen lässt, also wesentlich zur Sprache gehört, ist die Intension aussersprachlich (Propositionen, Eigenschaften, Individualbegriffe).

It is far too unclear what is meant by saying that meaning 'cannot be separated from word or sentence' (let alone that it 'belongs essentially to language') for one to judge whether this is a difference between Jánoska and Carnap or not. And if it is, it is strange that it is not also thought to distinguish Jánoska's from Frege's *Sinn* and *Bedeutung*, where the difference is said to be merely terminological.

Plato's theory of meaning is presented as arising out of the requirement that if we know what a word means we should be able to say. The author allows this requirement provided that 'saying what we mean' can include listing the cases in which we use the word, which he appears to equate with *how* we use it. But it is of course very doubtful whether simply listing the cases in which it is applied, without any indication of what features of those situations are the ones which make the application correct, can constitute saying what we mean *at all*; one can very well deny that it does without being committed to the view that saying what we mean can only be giving a definition *per genus et differentiam*.

There is too much of this sort of thing in the book. In Chapter VII Wittgenstein's attack upon the ideal of a perfectly precise language is forged into a weapon of a completely mindless nature—that of accusing opponents of generating problems by being 'over-exact':

> Von 'Übergenauigkeit' möchten wir also immer dann sprechen, wenn Definitionen gegeben, d.h. Grenzen gezogen werden, die durch den vorgegebenen Sprachgebrauch nicht begründet sind (p. 47).

This, and its application to a doctrine of Arthur Pap, read like a caricature of 'ordinary-language philosophy' at its worst.

Chapter VIII opens more promisingly with an account of the difference between the linguistic and the phenomenological approach. But it returns to normal with a criticism of Goodman's distinction of First and Second Extensions (pp. 59–60), which the author evidently holds to be not even a *prima facie* advance on the slogan 'Meaning is Use', as if the question 'What is it about the use which is important for meaning?' needed no answer.

So I am afraid that I cannot recommend this book as a whole to the English-speaking reader, unless he has a particular interest in the state of the subject in the German-speaking countries. The philosophical yield is not proportionate to the effort. But I found Chapter 9 ('Dialectic Logic and Contradiction') and Chapter 12 (about Heidegger's Nothing) interesting. Either of them can be read independently of the rest of the book.

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MELTZER, Bernard and MICHIE, Donald, Eds. (1969) *Machine Intelligence 4.* Edinburgh: Edinburgh University Press. £5 (100*s*.). Pp. viii+508.

This fourth volume of the *Machine Intelligence Workshop* series covers the general areas dealt with in its predecessors, and some of the twenty-six papers published here describe later stages of research projects initially described in those earlier volumes. The discussions of the mathematical foundations of programming and the mechanisation of theorem-proving should be of interest to logicians. And the papers on cognitive processes, pattern recognition, and the design of 'intelligent' robots, illustrate some of the psychological and philosophical problems connected with the computer simulation of behaviour.

The papers which I found of especial interest include one by *Doran*, which presents a developed version of an automaton he first described in volume III. *Kiss* gives a model of word-selection which simulates the probabilistic nature of word-association responses in the psychological laboratory. *Coombs* and *Hilditch* discuss the recognition of postal codes and chromosomes respectively; *Varshavsky* presents a mathematical discussion of the organisation of interaction in groups or 'collectives' of automata. And *McCarthy and Hayes* discuss mechanisable formalisms of causality, ability, knowledge and tense, which they compare with various formulations in philosophical logic.

Willshaw and Longuet-Higgins describe the theory of the holophone: this is a device analogous to the holograph, but working in time rather than in space. It has been suggested that the brain may store information holophonically, which would account for some of the puzzling properties of memory. The surprising lack of precise localisation of the memory-trace, which was suggested by Lashley's experiments on learning, is quite consistent with a holophonic storage-mechanism; and, just as we can recognise heard snatches of many different tunes and can then sing them ourselves on to the end, so also if any part of a stored message is the input to a holophone, the machine's output will be the full continuation of that message. Whether or not human memory can be explained holophonically, such a device could be useful in the mechanical storage and recognition of temporal patterns of stimuli, and could give rise to performances which have often been thought to be incompatible with mechanism.

Clowes' paper is a stimulating discussion of what he calls the 'syntax' of pictorial representation and visual perception. Clowes sees his enterprise as closely parallel to that of Chomsky: both are searching for the *deep* grammar, our intuitions of which determine our understanding of the expression we can readily describe in terms of its *surface* grammar. (And both believe that the system of attainable concepts may be limited by *a priori* features—whether perceptual or linguistic.) A 'picture-grammar' should characterise in some metalanguage all the many distinct relationships which we can readily identify and name in pictorial expressions, and it should formally define just those relationships which *in fact* mediate our grasp of the structure of the expression concerned. It should thus help to explain why we interpret particular visual patterns as we do. We may require not only careful phenomenological description of 'normal' visual patterns, but also the experimental use of particular 'anomalous' expressions (such as ambiguous figures or the visual illusions), if we are to render our deep intuitions clear-cut and obvious: duck-rabbits may be as useful as flying planes. If such a metalanguage could be produced, it should enable us to *state* the specific types of perceptual 'shift' or reorganisation involved in the familiar ambiguous-figures, figure-ground illusions and 'impossible objects'. It is often intuitively obvious that some such shift has occurred, and the Gestalt psychologists tried to characterise it rather more precisely in terms of concepts such as 're-centring' and the like. But the Gestalt concepts (and those of everyday phenomenological description) are still too vague; a full understanding of these perceptual phenomena would doubtless require some more precise terminology. Clowes suggests a skeleton meta-language, and tries to state our intuitions of form and position in terms of it. He also uses it to express certain deeper intuitions which he believes—on *empirical* grounds—to be involved in our visual perception: e.g. the use of implicit axes, the implicit assignment of an interval scale with a small (and relatively fixed) number of units, and so on. These basic relationships which, he says, we expect *a priori* to find in visual patterns, are perhaps less startling than some of the Kantian categories. But, if Clowes is right, they do determine our perceptual interpretations of form and objects on the basis of visual patterns, and they do set limits to the structure of the system of attainable concepts.

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√ FEIGL, H. (1968) *The 'Mental' and the 'Physical': The Essay and a Postscript.* University of Minnesota Press. 43*s*. (Paperback 17*s*. 6*d*.). Pp. vii+179.

This book is a reprint of Feigl's essay of the same title in Volume II of the Minnesota Studies in the Philosophy of Science with its extensive bibliography; to this is added a postscript written ten years after the original essay, together with an additional bibliography.

There can be no doubt of the value of the bibliography containing, as it does, between five and six hundred items. But it is surprising that the essay itself was thought worth reprinting. It is neither an introduction to the field, nor a survey of the various theories, nor an especially significant contribution to the 'problem'. Moreover it is written in such a turgid style that to grasp what Feigl is actually saying requires no small effort.

When he wrote the original essay Feigl advocated what he calls an identity theory. He claims that raw feels and neural states are to be identified, the identification being empirical and not in any sense logical. Thus he seems to be saying little more than that there are empirical correlations between the raw feels and neural states, correlations which he would agree will have to be established by science in the future; these correlations are to be called identities.

Apart from the rather strange and over-simple use of the term 'identity', there are other difficulties which Feigl does not face. To establish such correlations requires the clear identification of both elements in the correlation, and the method of identification of raw feels is not discussed sufficiently. Of course someone may say they are seeing something yellow, and we can infer from this behaviour that they are having a raw feel of a particular sort. But can the raw feel be made sufficiently specific to enable correlations to be established? Of course this is a traditional difficulty and not one for Feigl's view alone. It is not unconnected with the private language argument which Feigl dismisses in a foot-note to the Post-script. It is strange that he should neglect so completely the Wittgensteinian approach to this set of problems; not because it necessarily gives the answers, but because it so clearly raises fundamental questions. This neglect is perhaps the result of Feigl's logical positivist inheritance; though he rejects its conclusions, he nevertheless seems to accept its method.

No doubt Feigl is influenced by his interest in and knowledge of science, and sometimes he seems to be writing as a philosophically naïve scientist might write about these problems. I would not disagree with his view of the interconnections of science, or at any rate, empirical investigation, and philosophy; but Feigl does not really discuss the possibility that the scientific investigation of the facts he is interested in may presuppose a certain view of the 'mental' and the 'physical'. There is some doubt whether the empirical evidence which he wants to support his view is as conceptually neutral as he seems to suppose.

In the Postscript, Feigl claims to revise his identity view, but it is difficult to see in what way he revises it. The ten years that have elapsed since the original essay do not seem to have increased Feigl's clarity of expression. He does make one significant remark in the Postcript: 'Although I have focussed my intention primarily on the sentience problem, I regard the others as equally important. But I must confess that, as before, the sapience and selfhood issues have always vexed me less severely than those of sentience.' Perhaps if Feigl had seen that the issues were not so easily separated, and indeed that the answers to questions related to sentience may presuppose answers to the other issues, then he might have progressed further.

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FRANKLIN, R. L. (1968) *Freewill and Determinism: A Study of Rival Conceptions of Man.* London: Routledge. 45*s*. Pp. x+346.

In a book that would have been the better for rigorous pruning, R. L. Franklin argues that metaphysical theories are unprovable, but that each of opposing views can be rationally supported by appeal to its congruence with a broad, coherent view of man and the universe. Clarification achieved by analytical philosophy illuminates the freewill issue as thus lying between alternative basic presuppositions. Franklin prefers Indeterminism as the 'natural' view of choice, alone consistent with a serious deontological ethic.

In presenting the freewill issue as indissoluble, Franklin has continually to recur to the claim that a normal, thoughtful man will, in choosing, regard both alternatives as 'ineluctably open' (p. 68). Now certainly free choice is normally thought to be between alternatives that are open (and doubtless ineluctably so), but might not the concept of *openness* simply be unrelated to that of causal explicability? Franklin's response to a similar suggestion about *possibility* is peculiar. The 'personal', 'operative' *can*, he concedes, presupposes neither indeterminism nor determinism, but he argues that the natural indeterminist intuition still stands. Thus, uniquely as I imagine, he allows that both determinist and indeterminist can consistently apply the personal 'could have chosen otherwise' in the same, normal, categorical sense, and still have something worth quarrelling about in relation to responsibility. One wonders why 'The alternatives are ineluctably open' does not merit the same treatment.

The best argument in the book demolishes Campbell's Libertarianism (Appendix I), but the proffered alternative, which locates indeterminacy in the selective directing of attention, while allowing for responsibility for thoughtlessness, and for freedom in non-moral choices, is otherwise unattractive, ascribing to us a more direct control over our thoughts than over our actions.

Franklin's efforts to represent Determinism as both coherent and morally significant are equally unpersuasive. For example, the attempt to link it with an ethic of consequences fails to distinguish adequately between the consequences of the act assessed and the consequences of expressing the assessment. He also seems to think that a consequentialist, in assessing an act, must ignore its intention. Nevertheless he presents some acceptable argument against the Schlick-Stevenson approach, and elsewhere there are discussions of interest, if generally lacking in bite.

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WILLIAMS, L. Pearce (1968) *Relativity Theory: Its origins and impact on modern thought.* New York: Wiley n.p. Pp. viii+159

This is one of a series of individual volumes entitled 'Major Issues in World History'. The series volumes have been edited by history scholars, and as the senior editor of the series explains in the preface, their themes and titles have been determined by two criteria: (1) that the issue be of relevance to undergraduate lecture courses in history and (2) that it be an issue which the scholar editor knows thoroughly and in which he has done creative work. Thus special relativity finds itself side by side with such other 'major issues' as the Norman conquest and the twelfth-century renaissance.

The book consisting of relevant extracts from the works of scientists, philosophers and laymen, is divided into four parts. There is a brief commentary by the editor before each extract giving the historical background and the general theme of the work.

Part I deals with 'the Origins of the Special Theory of Relativity'. It begins with an extract from Newton's *Principia* describing the Newtonian concepts of space and time, of absolute and relative motion. This is followed appropriately enough by Mach's criticism of Newtonian views as given in the *Science of Mechanics*. We then have Michelson & Morley on their famous experiment. As the editor points out, this experiment is more often cited than read today. The original paper describing the experiment is an interesting part of this collection. The implications of this experiment for theoretical physics are discussed in the following two contributions from Lorentz and Poincaré. Finally we have Einstein's paper which first spelled out the outlines of the Special Theory of Relativity. However, only the non-mathematical part of the paper is given.

The second part is about the nature of relativity theory and contains only one article—that by its creator. This is a popular account of both the special and general theory written in 1921. Part II contains articles by Einstein, Whittaker, Holton and Grünbaum about how the theory of relativity was born. Einestin's contribution is taken from his famous 'autobiographical notes'. The extract from Whittaker's *A History of the Theories of Aether and Electricity* describe the events that led to the theory from the eyes of a contemporary physicist. We then have the points of view of a historian (Holton) and a philosopher (Grünbaum). How much, if at all, was Einstein helped by the earlier work of Poincaré and Lorentz? It was inevitable that a theory which created such a profound stir in physics should evoke such controversies. They make interesting reading to the student of the history of science.

The final part is about the impact of the theory on the world at large. First we have the scientific reaction in terms of the extracts from the works of Magie, More and Eddington. The extracts indicate, in a small measure, how different scientists received the theory in different ways. Next comes the lay reaction. The theory of relativity has been unique in its impact on the lay public. It is clearly impossible to even summarize the vast amount of lay literature on the subject. The selection given in the book, including an editorial from the *New York Times*, gives some idea of the diversity of the lay reaction.

This book will make a valuable addition to the library of the student of history of science. Some of the extracts are too short and sometimes fail to convey to the reader the full impact of the complete articles. This, perhaps unavoidable shortcoming, is corrected to some extent by the inclusion of a select bibliography at the end.

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Abstracts

Synthese, 20 (1969), No. 1

de FINETTI, Bruno
Initial probabilities: A prerequisite for any valid induction

Inductive reasoning necessarily requires, beside the knowledge of observed facts, some other underlying assumptions.

These assumptions are disguised, by objectivists, in the form (meaningless for subjectivists) of seemingly objective statements such as that certain 'hypotheses' are the 'possible' ones or certain events are 'independent', of 'Bernoullian', or certain random variables are 'normal', and so on. But such rigidity is unsuitable, and is in fact abandoned whenever the conclusions seem unsatisfactory.

To remedy this inability, the assumptions must be interpreted in the more supple form of a (subjective initial) probability distribution over the space of the 'hypotheses' (a 'middle-of-the-road approach') or, better (the 'radical approach'), formulating the same assumptions with reference not to metaphysical 'hypotheses' but to observable events only, according to the requirements of strict reductionism.

GOOD, I. J.
Discussion of Bruno de Finetti's paper 'initial probabilities: a prerequisite for any valid induction'

By an 'extreme Bayesian', such as de Finetti, is meant one who believes that the only kind of probability is intuitive probability (subjective or logical) and that physical probability can be *defined* in terms of it. On the other hand the author is less extreme since he believes that physical probabilities seem to exist independently of intelligent entities but that they cannot be *measured* without the help of subjective probability. His argument depends in part on two 'postulates of impotence'. He gives also a brief outline of his views on induction.

HACKING, Ian
Linguistically invariant inductive logic

LEVI, Isaac
Confirmation, linguistic invariance and conceptual innovation

In his paper 'Inductive Logic, Part I', Ian Hacking suggests a way of eliminating the sensitivity of Carnap's confirmation measures to changes in languages involving changes in the way families of predicates are partitioned. If Hacking's version of confirmation theory is to be used to determine fair betting quotients in a manner similar to that envisaged by Carnap, his proposals lead to inconsistent assignments of fair betting quotients. The contradictions can be eliminated, however, if Hacking's proposals are not regarded as part of an effort to insure linguistic invariance but are taken to be a first step in an account of how conceptual changes induced in Quinean fashion by changes in evidential beliefs alter confirmation functions.

VETTER, Herman
Logical probability, mathematical statistics, and the problem of induction

Mathematical statistics, based on statistical (physical) probability, can solve inductive problems only if a representative random sample is present, which is not the case in most scientific problems. Inductive logic, based on logical probability, might be useful under the following conditions: (1) Inductive inference is construed as formally congruent with mathematical statistics, the only difference being the principle of indifference, or representativeness by fiat, used by inductive logic as soon as no evidence to the contrary is present; (2) the formal apparatus of inductive logic is developed far more than it is at present, so that it can handle complicated networks of many observations and several theories which are dealt with intuitively by scientists now. Even under these conditions, logical probability calculus will not be able to 'solve the problem of induction'.

HINTIKKA, Jaako
Statistics, induction, and lawlikeness: comments on Dr Vetter's paper

Two interrelated topics treated by Vetter are selected for examination: (1) the relation of theoretical statistics to philosophical and logical theories of induction; (2) the problem of inductive generalisation. Vetter's claim that a special case of the problem of induction is solved by the methods of mathematical statistics and his use of the notion of randomness are criticised. It is argued (somewhat like Savage and Simon) that traditional statistical techniques fail to give a satisfactory account of extreme hypotheses (e.g. strict generalisations). Hintikka's two-dimensional continuum of inductive methods is mentioned as a step toward a theory of inductive generalisation. The use of the parameter α is explained as a way of codifying assumptions concerning lawlikeness, thus bringing inductive logic to bear on this important notion. Finally, two specific criticisms of Vetter's are answered.

von KUTSCHERA, Franz
Zur Problematik der naturwissenschaftlichen Verwendung des subjektiven Wahrschkeinlicheitsbegriffs

In dieser Arbeit sollen die beiden Hauptbedenken, die gewöhnlich gegen die Verwendung des subjektiven Wahrscheinlichkeitsbegriffs in den Naturwissenschaften erhoben werden, kritisch untersucht werden. Diese Bedenken richten sich dagegen, dass subjektive Wahrscheinlichkeitsaussagen keine objektiv gültigen Aussagen sind, sondern nur kraft subjektiv—willkürlicher Setzung gelten, und dass sie keinen empirischen Charakter haben, also nichts über die Welt aussagen. Als Grundlage der Diskussion wird zunächst eine kurze, nicht formale Charakterisierung sowohl des objektiven wie des subjektiven Wahrscheinlichkeitsbegriffs gegeben. Dabei ergibt sich, dass ein befriedigender und geschlossener Aufbau der objektiven Wahrscheinlichkeitstheorie nicht auf den subjektiven Wahrscheinlichkeitsbegriff verzichten kann. Daher beträfen die beiden Einwände, wären sie stichhaltig, nicht nur den subjektiven, sondern auch den objektiven Wahrscheinlichkeitsbegriff. Weiterhin ergibt sich aber, dass auch subjektive Wahrscheinlichkeitsaussagen durch objektive, d.h. intersubjektiv gültige empirische Argumente überprüft werden können—zwar nicht, was ihre Wahrheit oder Falschheit anbelangt sondern bzgl. ihrer Angemessenheit an ein gegebenes Erfahrungsdatum—so dass sie bei einer naheliegenden Erweiterung der Begriffe 'intersubjektiv überprüfbar' und 'empirisch' als objektive und empirische Aussagen angesprochen werden können.

FREY, Gerhard
Über die gültigkeit genereller Sätze

Im Anschluss an die bekannte Feststellung von Quine, dass eine strenge Unterscheidung von analytischen und synthetischen Sätzen nicht immer möglich ist, ergibt sich, dass auch Erfahrungssätze einen Geltungswandel unterworfen sein können. Dies gilt insbesondere, wenn man Sätze, die analytisch sind relativ zu einer Begriffs - oder Definitionsbasis, in Betracht zieht. Die Überprufüng empirisch genereller Sätze erweist sich als abhängig nicht nur von Erfahrungen, sondern auch von Konventionen. Durch diese können Begriffe abgewandelt werden. Analoge Verfahren lassen sich auch bei der Anpassung und Abwandlung von Theorien festellen. Die Fortschritte der Wissenschaft vollziehen sich durch konventionale Anpassung der Sprache an die Erfahrungen.

BAUMER, William
In defence of a principal theorem

This essay first proposes an eliminative confirmation interpretation of the Principal Theorem of Confirmation and of a Converse Principle Theorem. On this approach an hypothesis is confirmed to the extent that its plausible alternatives are eliminated as false or over-qualified; it is disconfirmed but not falsified by evidence which introduces additional plausible alternative hypotheses. Various bases for the determination of such plausible alternatives are indicated. On this foundation three problems posed for the Principal Theorem are answered. Evidence decreasing the degree of confirmation of an hypothesis is handled by the Converse Principal Theorem. There is no problem of reconciling the counting of instances of hypotheses with the counting of hypotheses in determining confirmation values since instances *per se* are not counted, elimination values are. Since only eliminative instances are confirming, this approach answers the question of which consequences of an hypothesis provide supporting evidence for it and which do not. The essay closes with comments on possible further developments of the eliminative confirmation approach.

Synthese, 20 (1969), No. 2

HULL, David L.
What philosophy of biology is not

Contributions of philosophers to the philosophy of biology during the past ten or fifteen years are reviewed and criticised. In the first half of the paper, the implications which philosophers have seen in biology for philosophy are discussed—primarily the consequences of evolutionary theory for man. In the second half of the paper, various philosophical analyses of biology are investigated to see if they provide any insights or clarity superior to those provided by biologists themselves. The general conclusion reached is that most philosophers who attempt to discover the significance of biology for philosophy are too often totally ignorant of biology and those who attempt to apply the tools of philosophy to biology have seldom come up to the level of sophistication attained in the biological literature.

MANIER, Edward
The experimental method in biology

The work published by T. H. Morgan in the interval 1900-10 (the decade prior to his discovery of the chromosome theory of the gene) suggests interesting variations of several issues familiar to philosophers of science. Most important is the interplay of theory and experiment in his thought, particularly the significance of his sustained defence of an epigenetic theory of heredity in opposition to Mendelism. The plausibility of several theoretical alternatives in genetics led Morgan to regard scientific explanations as fictional or arbitrary unless they gave rise to consequences testable by independent methods.

Conclusions are also drawn concerning the significance of studies in the history of biology for such problems as functional analysis and reductionism.

MANIER, Edward
'Fitness' and some explanatory patterns in biology

The meaning of 'fitness' as a biological term is relevant both to disputes over the logical pattern of functional analysis and to the effort to find a common ground for the molecular biologist and the evolutionist. Analysis of the patterns of teleological explanation in biology has generally been thought to show that these patterns have extremely limited generality and lack predictive significance. Among the most plausible foundations for this claim are the views that 'fitness' must refer both to adaptation (roughly: specialisation) and adaptability (roughly: generalised flexibility) with paradoxical implications for any general fitness strategy; and that 'fitness' can be empirically specified only in terms of reproductive success so that formulas including it cannot be independently tested. These views are critically assessed in terms of recent studies in molecular genetics and population genetics.

ACKERMANN, Robert
Mechanism, methodology, and biological theory

This paper argues for two major points about the methodology of biology. In the first part of the paper, it is argued that there are logical properties of classes of biological entities which defeat any easy transfer of a methodology dependent on the projection of properties from samples to whole classes from the domain of physics. In the second part of the paper, a position called *indeterministic mechanism* is sketched which avoids arguments raised against vitalism while at the same time exhibiting consistency with the anti-reductionist consequences of the methodological issues discussed in the first part.

CAUSEY, Robert L.
Polanyi on structure and reduction

Michael Polanyi has recently argued that biology is not reducible to physics and chemistry. Polanyi's arguments do not quite establish his thesis, but some of his points, particularly about structures, are worthy of serious consideration.

The first section of this paper contains some general remarks about structures and reduction. I distinguish two types of explanations of structure, and also distinguish *direct* from *indirect* reductions.

The second section of the paper contains an analysis of Polányi's argument in the light of section one. I try to show exactly what Polanyi's arguments accomplish, as well as show that they do not prove his irreducibility thesis.

HEIN, Hilde
Molecular biology *vs.* organicism: the enduring dispute between mechanism and vitalism

The controversy between molecular biology and organicism is a modern descendant of the dispute between mechanism and vitalism. Unlike opposing scientific theories, their disagreement is not primarily over substantive issues, but is reflective of primary commitments on the part of the disputors. Disagreements of this type characteristically involve (1) a strong polemic component, (2) a basic concern with methodology, (3) rejection of proferred evidence as irrelevant or meaningless, and (4) a defence of the favoured position on heuristic grounds. The two modern positions are compared in terms of these criteria, and they are related to the older doctrines.

LAGERSPETZ, Kari Y. H.
Individuality and creativity: is biology different?

The arguments for vitalism presented by W. M. Elsasser are discussed. There is individuality in non-living objects, e.g. in geological and geophysical objects. The classes of such objects are inhomogeneous and finite. However, biology deals with its objects, as do the physical sciences, by the abstractive method. The creativity which appears in ontogenetic development does not give a valid proof for the lack of information conservation. The creativity also appears in complex non-living systems and is also studied by methods of abstraction. The objects and methods of biology are thus not different from those of the physical disciplines dealing with complicated systems.

MACKLIN Martin and MACKLIN Ruth
Theoretical biology: a statement and defence

Theoretical biology is characterised as the application of propositions, techniques, and procedures from mathematics and symbolic logic to biological phenomena at all levels. In response to several lines of attack, the enterprise of theoretical biology is defended in the paper. Three representative objections are considered, each of which presupposes some conception of the nature of the science of biology. It is argued here that it is a mistake to rule in or rule out, in principle, some particular conception of the nature of biological inquiry. Theoretical biology is preferable to a purely descriptive approach to biology, on grounds of its greater systematisation and fruitfulness for further inquiry.

ROLL-HANSEN, Nils
On the reduction of biology to physical sciences

On the background of a strongly reductionist climate in modern biological research, the author analyses philosophical theories of reduction, of the type proposed by Ernest Nagel. These appear to be based on a tacitly presupposed epistemological primacy of physical science, neglecting the radicality of change inflicted upon all sciences by historical development. An up-to-date justification of this primacy is wanting. It is also pointed out that many of today's arguments for the *non*-reducibility of biology to physical science are based on the same epistemological asymmetry.

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Causality and Statistics in Modern Physics[1]

by HERBERT DINGLE

THE PHILOSOPHY of science—or at any rate the philosophy of physics—has thrust itself on general attention during this century mainly through two developments—the theory of relativity and quantum theory—and of these, quantum theory, if not the more fundamental, is certainly the more complex and gives rise to the greater variety of problems. It cannot be said that the success achieved with the philosophical problems compares favourably with that attained in the purely physical investigations, and it may perhaps not unfairly be said that the naive realism of the nineteenth century has shed its realism more thoroughly than its naiveté. However, this was perhaps inevitable, though it is none the less regrettable that some of the more extravagant generalisations from the use of the mathematical theory of probability in physics have received such publicity and have been invested with prerogatives that rightly belong only to the mathematical theories within their own sphere.

Among the attempts which have been made to interpret the new outlook, that of Professor Bohm seems to me to have special significance, derived not only from the exceptional clarity of his exposition but also from that of the underlying thought. It has a further merit which, so far as I know, it shares with only one other philosophy—that of Eddington. Whereas most philosophical disquisitions on the quantum theory are one-way processes, in which the scientific theories are taken as unquestionable data for philosophical meditation which can result in little, if anything, useful to science, Bohm uses quantum theory as a means of attaining a viewpoint from which physics itself can be surveyed to better advantage and lines of further scientific progress can be discerned. This is surely the

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[1] This paper was written a good many years ago, as a result of reading Professor Bohm's book which it purports to discuss. I submitted it to Professor Bohm for criticism, but found that he was not satisfied with the statement of his views that it contains. Notwithstanding a lengthy correspondence, however, I was not able to convince myself that the scheme of thought I had ascribed to him was not entirely compatible with the contents of his book. Nevertheless, to avoid profitless controversy I put the paper aside. Subsequent correspondence with others on its general theme now makes me think that publication of the paper might serve a useful purpose, but I wish to state emphatically that Professor Bohm should not be credited with any ideas that I have here fathered on him, without verification from his own writings. Whatever their parentage, those ideas seem to me to be of great interest and worthy of the fullest consideration.

right attitude. Science and the philosophy of science are, properly speaking, inseparable, and the ideal point of view has not been reached so long as "soul helps flesh more now than flesh helps soul".

The purpose of this paper, however, is not to discuss the scientific suggestions, interesting and cogent though they are, which Bohm's philosophy leads him to advance. They are in course of development, and in due time will doubtless lead to the formulation and performance of experiments which will test their value. What I propose to do here is to outline the scheme of thought which he presents in his (1957) book, and to examine it from a somewhat different point of view.

Bohm's thesis, as I understand it, is as follows. There is an objectively real world which consists of an infinite variety of things having an infinite variety of qualities. (I think the only indefiniteness which I have found in an otherwise admirably clear and unambiguous statement is the tendency to give certain terms an embarrassingly wide connotation. Thus "things" are defined as "properties, qualities, entities, systems, levels, etc.", and "motion" as "not only displacements of bodies through space, but also all possible changes and transformations of matter, internal and external, qualitative and quantitative, etc." This necessarily tempers my confidence that I have fully grasped his meaning in all respects. I have no doubt, however, from the general characteristics of his book, that his own ideas are perfectly clear, so I regret all the more that he has placed this obstacle in the way of their transmission). This infinite variety, however, forms a unity, described as 'the infinite totality of matter in the process of becoming'. In other words, the universe is a universe—i.e. its parts are interconnected—but its complexity is such that the whole "can be represented only with the aid of an inexhaustible series of abstractions from it, each abstraction having only an approximate validity, in limited contexts and conditions, and over periods of time that are neither too short nor too long".

The abstractions which we make in physics may be represented in this way. If this sheet of paper (supposed infinite in extent) represents the totality of matter, then we may draw a succession of horizontal "levels" or "domains", each of which may be regarded as approximately autonomous, but, when finer details are taken into account, as influenced by conditions in the neighbouring levels. For example, the level *A* (Fig. 1) is the *macroscopic* level (and will be so called hereafter), in which the entities considered are ordinary objects such as stones, trees, stars. These, to a high degree of approximation, obey the ordinary mechanical laws, but in special circumstances they exhibit phenomena (such as the Brownian movement) which are unintelligible in terms of the entities in this level alone, and to account for them we have to appeal to the level B, the molecular level. Here the entities are molecules and atoms of various kinds, and again we have an approximate autonomy, which led nineteenth-century physicists to regard the units in this level as the eternal foundation stones of the physical universe.

But here also we meet with certain phenomena—e.g. the emission of spectra, ionisation of gases, etc.—for the explanation of which we are forced to descend to a level *C*: here belong sub-atomic particles—electrons, protons, photons, etc.. As Bohm sees it, there is no necessary limit to this succession of levels, and we must be prepared to discover level after level, in unending succession, as we probe deeper and deeper into the secrets of nature.

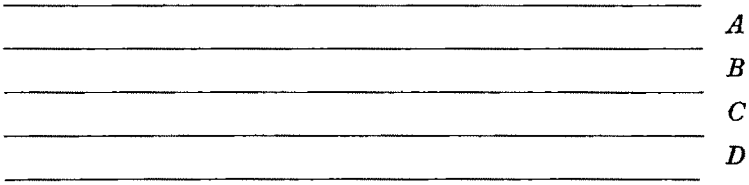

FIG. 1.

So much for the structure of the world: the next question concerns the character of the laws which we find operating at the various levels. These are of two kinds—*causal laws* and *laws of chance*—and both operate in every level. Causal laws are laws of necessity; e.g. a causal law says that, given such and such conditions, one particular result will necessarily follow. We find by experience that causal laws do operate, but never with absolute precision. All that a causal law can tell us with certainty is that a result lying within a small range of possibilities will be realised. This is because, as well as the causal law, a law of chance operates. This implies that every event in a given level is subject to an influence, indescribable in terms of the characteristics of that level alone, which modifies to an extent not precisely predictable the expectation arising from the operation of the causal law. This influence, when considered in relation to a single event on the smallest scale in a given level, can merely be called chance, but, when a large number of events are considered, the individual fluctuations are found to neutralise one another, so that, notwithstanding their individual arbitrariness, they are subject to a *law* of chance which ensures that the deviation from the requirements of causal law shall be limited so long as events on any but the smallest scale (smallest, that is, in the context of the level in question) are under consideration.

The distinctive feature of Bohm's view of natural law is this: that whereas in classical and in orthodox quantum physics the aim has been to reduce one of these two types of law to the other, he regards each as having independent authority, but as modifying to some extent the operation of the other. Thus, in the macroscopic domain, the causal mechanical laws are approximately valid on the large scale because of the operation of the law of chance in cancelling out the effects of small individual fluctuations. Nevertheless, since the cancelling out is never perfect, the causal law is never exactly obeyed. On the other hand, the fluctuations which are the subject-matter of the law of chance are not completely arbitrary but are fluctuations about a mean which is determined by causal law, so that, for instance, although any individual molecule of air may travel in any direction, the density of air near the ground is greater than that at a higher level.

The philosophy which assumes that there is a definite lowest level, in which only one of the two types of law operates, Bohm describes as *mechanism*—deterministic mechanism if the fundamental law is causal, and indeterministic mechanism if it is a law of chance. In contrast to this, he proposes a possibly infinite series of levels, in each of which both causal and chance laws operate, though the actual laws that do so may vary from level to level. So long as we are satisfied with approximations, each level may be regarded as completely describable in its own terms. Thus, in the macroscopic domain we may describe the movements of the planets in terms of causal law, and the uniform greyness of a thoroughly executed mixture of black and white particles in terms of the law of chance applied to the movements of those particles; but when we take the Brownian movement into consideration, the contents of the macroscopic level fail us and we must appeal to the next level—that of molecules and atoms. In terms of the contents of the macroscopic domain, the behaviour of molecules is indeterminate; nevertheless, the movement of each molecule, that manifests itself as a "chance" movement of a *macroscopic* spore, may itself be describable in terms of a mainly causal law operating in the *molecular* domain. But since in that domain also there are both chance and causal laws, the occurrences there cannot be completely described without descending to the next level—that of the so-called fundamental particles. So we proceed, possibly without limit.

In contrast to this view, the currently orthodox interpretation of quantum theory is a form of indeterministic mechanism, for it supposes that we have reached a fundamental level of entities whose individual behaviour is *essentially* indeterminate: that is to say, not only are we unable to determine it now or at any other time, but it is a part of the definition of the entities that certain essential features of their make-up are intrinsically indeterminable. Just as it belongs to the nature of a circle that all points on the circumference are equidistant from the centre, so it is a part of the definition of an electron that in certain circumstances the probability that it will behave in a certain way is, say, 1/5; and it would be just as meaningless to try to account for this in terms of something more fundamental as to try to explain why all the radii of a circle are equal. Bohm opposes this view, first by claiming that it is essentially unscientific, for it can be at best merely a supposition which can never be proved but might very well be disproved, and secondly by seeking a sub-quantum level in which entities behave in such a way as to produce the probabilities in the quantum level which orthodox theory regards as irreducible. With the second endeavour we are not here concerned: our interest is in the opposing views of the nature of science.

First of all, I should say that, if the pre-supposition which both sides seem to accept be granted, then it seems to me that Bohm's view is undoubtedly correct. That pre-supposition is that all the entities considered—macroscopic bodies, molecules, atoms, electrons, photons, mesons, hyperons and what not—are ultimately of the same epistemological character; they are all objectively existing elements of nature whose existence, character and behaviour we try to discover. If this be granted, then clearly we can set no *a priori* limit to what nature may contain or manifest. We are seekers, and to say that nothing can exist or occur other than what we have already found is to assume an omniscience which is a denial of the basic principle of science.

But I think there is more in the matter than that, for the pre-supposition that the structure of modern physics consists entirely of objective entities whose character and inter-relations we merely bring to light is, I think, quite false. It seems to me that if we substitute for that a view more nearly in accord with the actual process of scientific research, the orthodox interpretation presents a much more favourable aspect, and I believe that in fact orthodox physicists are implicitly adopting that view, notwithstanding the philosophy of physics which they might formally profess if called upon to do so.

Put summarily, I should say that the aim of *science* would be completely achieved if we could say what we should experience in any given circumstances. The aim of any *experimental* science would be achieved if we could say what we should experience if we *did* some particular thing. The aim of *physics* would be achieved if we could say what we should experience if the particular thing in question were the making of a measurement.

We are concerned now only with physics, and I think that the statement which I have given, bare and restricted though it is compared with the instinctive view which I am discarding, can be seen to be true when we examine the achievements of physics up to the present. They are all summed up in equations,[1] and every equation is composed of terms, each of which, whenever the equation is applied to a particular case, stands for the result of a measurement or a combination of results of measurements. I think this is true without exception. Eddington asserted that the subject-matter of science was pointer-readings. I think this was far too sweeping a statement, but it is literally true of physics.

I know, of course, that this is an absurdly inadequate account of the actual prosecution of physical research. No progress would ever be made if the dry bones of pointer-readings were not clothed by imagination with vivid pictures of bodies behaving in certain ways and interacting with one another according to their nature. But although all this is necessary to inspire the physicist and to direct him along the paths that lead to success, his success, when it is achieved, is wholly contained in the equations which tell you what particular numbers you will obtain when you make certain specified measurements.

Since nothing in nature ever measures itself, it follows that the whole of physics is concerned with things that would never happen if we did not make them happen. As a means of predicting what will occur in nature, apart from the purposeful activities of man, physics is completely sterile, except for a purely empirical association which I will mention presently. Physics is entirely a science of the artificial: if there were no physicists there would be no physics—not merely no knowledge of physics, but no physical relations to know.

This is so much at variance with what is usually spontaneously assumed that it needs a little clarification. Surely, it will be said, we do predict natural occurrences—solar eclipses, for example—which would occur and would be observed if we never measured anything. True; but what we predict are the relative positions of what we *call* Sun, Earth and Moon, but are in reality the centres of gravity of those bodies, for those are the quantities that are represented by the symbols in our dynamical equations. An alignment of centres of gravity does not produce the phenomena of an eclipse, and the fact that they happen to be surrounded by opaque spheroids of particular sizes is something with which the equations have no concern at all. Moreover, if any of the bodies should explode through internal stresses, there would be no such phenomena: nevertheless the alignment of the centres of gravity would still occur. *That* is what we predict, and that is a relation between pointer-readings which is verifiable only by means of pointer-readings. The association of those particular pointer-readings with the descent of darkness at a particular time and place is purely empirical; it is not contained at all in the equations.

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[1] I use the word 'equation' in a very general sense, to include inequalities, such as the law of entropy, and approximate equalities, such as the uncertainty formula. 'Relation' would be in some respects more accurate than 'equation', but it would be far too wide, since many relations are non-metrical. I think the meaning will be clear enough.

Or, to take another example, if a prism is placed between the eye and an object, the refraction formula for the prism tells us into what direction the radiation from the object is deviated. We then find, in many cases, that if we look in the new direction we do, in fact, see the object, but the refraction formula does not tell us that. It gives us the pointer-reading of our instrument when it is placed so as to receive radiation of a particular wavelength. It then happens that if that wavelength is 5000 A (green) we see the object, but if it is 3000 A (ultra-violet) we do not; and there is nothing whatever in the formula to distinguish between the two cases. The formula is entirely a relation between a number of pointer-readings, and as such can be verified as often as we wish; but whether we see or not is a matter which, so far as physics is concerned, is completely unpredictable.

So it is with every physical formula: it predicts what measure-number we shall get in prescribed circumstances, but any other observation that may happen to accompany that of the measure-number does so for no reason at all within the scope of the formula. We just notice the association and take advantage of it, but of why it should exist we have not the slightest physical explanation.

I think the description of the aim of science as the determination of what we shall experience in any given circumstances is more accurate and more comprehensive than that which presents it as the discovery of what things the world contains and how they are related with one another, because anything in the latter that does not contribute to the former is merely metaphysical: if the former aim were realised completely, there would be nothing of a scientific character left to wish for. Let us, then, look at the structure of levels envisaged by Bohm from the two points of view, remembering that we are not now dealing with science in general, but only with physics, in which the relevant observations are all measure-numbers.

The first thing to notice is that when we regard all the entities concerned—macroscopic objects, molecules, atoms and soon—as fundamentally equivalent, having the same kind of objective existence, we are naturally led to expect that all the levels will be similar in their general character: each contains its own type of entities, obeying its own kinds of causal and chance laws, and each is related to the next lower level in the same general sort of way as any other. True, as Bohm points out, it is not necessarily a case of big fleas having little fleas upon their backs to bite 'em, and so *ad indefinitum*—first, because the levels do not necessarily all consist of fleas, and secondly, because we cannot presume that the succession must inevitably extend to infinity. Nevertheless, it is supposed that the domains all contain insects, and if there is in fact a final domain, then mechanism (deterministic or indeterministic, according to the kind of law operating there) is established after all. We must surely suppose that, even in Bohm's world,

> Big fleas have little nits upon their backs to bite 'em,
> These have still smaller lice, and so *ad infinitum.*

But in the actual world this is not at all the case. The macroscopic level appears to be unique in two ways, and this even from Bohm's own point of view. In the first place, it is the starting-point of the whole business, for although there are many domains below it, there are none above. We could, of course, make one higher domain called *the universe*, and this might claim some support from the fact that there are cosmological theories in which the universe is held to possess intrinsic properties, over and above the properties of its constituent macroscopic bodies. But in fact Bohm does not postulate such a domain, and in any case it is purely hypothetical because of the second characteristic of the macroscopic level—namely, that *all* observations, no matter to what level they refer, must be conducted there. We speak colloquially of observing single atoms, and discuss the possibility of observing single photons and the impossibility of observing single electrons, and so on, but I think I am right in saying that every observation that is described as an observation of an atom or still smaller particle is in fact an observation of a macroscopic object which is regarded as evidence of the functioning of such a particle. Wilson chamber photographs, for example, are observed as a train of silver grains, each containing many millions of atoms, and it is only by a process of reasoning that we can express this in terms of the behaviour of a single sub-atomic particle.

As I say, I believe that this is true of all such observations, although this question deserves closer examination than it is possible or necessary to give it here. For instance, in the experiments said to demonstrate the visibility to the eye of one or a few quanta, there is a subtle change of meaning of the word 'observation' which is not usually noticed. When we look at a table we say we observe the table, not the millions of photons which are supposed to proceed from it to the eye. But when we look at a very faint source of light we speak of observing the photons, not the source from which they come. This, I think, is very relevant in relation to the impossible thought-experiments often cited in support of the uncertainty principle. But for our present purpose this question does not arise because, as I have explained, all the observations that belong to physics are those of the scales of measuring instruments, and beyond all question these are macroscopic observations, conducted entirely in the macroscopic level. Accordingly, that level, so far as physics is concerned, is absolutely unique. To verify a law of the macroscopic level, no matter what law it may be, we must observe in the macroscopic level; but to verify a law of the molecular or electronic or any lower level, we must observe not in that level but again in the macroscopic level; and if we postulate laws of the universe, we can again verify or falsify them only by observations in the macroscopic level. No one can observe the curvature of the universe to see if it is positive or negative or zero: whatever conclusions he reaches on that point must rest ultimately on observations in the macroscopic level alone.

Now all this is entirely conformable to the view that physics is the discovery of what pointer-readings will be observed in any stated circumstances. From that point of view, the circumstances of the observation are all completely describable in terms of the entities existing in the macroscopic domain, and the whole content of the other domains—entities, relations, laws and so on—becomes purely conceptual, the rational machinery by means of which we elucidate the macroscopic world. If there is an error in it (other than purely logical mistakes, of course) that error can be discovered *only* by the fact that it leads to macroscopic expectations that are not realised. It is, in the nature of things, for ever impossible to discover it in any other way.

The question whether we should regard the indeterminacy of the entities in any of the lower levels as final or as explicable in terms of still lower levels, now appears in quite a different light. When we suppose such entities to be objective bodies, existing in the same sense as our measuring instruments exist, then it is at least aesthetically unsatisfying to suppose that they possess an inherent indeterminacy, and certainly inexcusable to prohibit the reduction, if possible, of such indeterminacy to the determinable effect of still lower level entities. But when the lower levels are seen as purely conceptual, the only question that is apposite is whether the conceptual scheme as a whole serves the purpose of predicting observations in the macroscopic domain. In that case, if the concepts in a certain level, when accorded a property called *probability*, do serve the purpose of issuing ultimately in trustworthy predictions of observations, it is a mere waste of time to try to reduce that probability to the certainty of behaviour of still other conceptual postulates. They are not given us to interpret; they are invented by us for a particular end, and if they serve that end there is nothing more we need ask of them.

From this point of view, therefore, I see nothing to which objection can be taken in the description of the uncertainty relations, say, as fundamental in physical theory—though, I repeat, I am expressing no opinion whatever on their validity, i.e. on the actual effectiveness of the uncertainty relations in serving the purpose of physics. But it does seem to me, nevertheless, that the customary arguments for the uncertainty principle indicate that its advocates do not, in fact, adopt the standpoint from which alone it can be justified. The hypothetical experiments, in which one is supposed to try to observe an electron by directing a photon on to it, seem to me to show such confusion that I can hardly conceive how they could ever have come to mind. Even accepting the view that an electron is an objective, potentially observable, object, you must first of all know where it is before you can aim the photon at it, and in that case you have no need to make the experiment. But, having done so, you must suppose that the photon, although it knocks the electron into some quite indeterminable position, itself obliges you by proceeding from the microscope to your eye, so as almost to tell you where the electron was. But it is superfluous to analyse the procedure in detail because, as we have already seen, the only relevant observation in the whole business is the reading of the scale of the instrument when it is placed in specified circumstances. In this hypothetical experiment as customarily conceived, you cannot, in the nature of things, specify the circumstances, and the reading, whatever it may be, has no relation to them.

The only philosopher of science, so far as I know, who has realised the necessary implications of regarding the present quantum level as final, is Eddington (1939). Though by no means a typical quantum physicist, he nevertheless accepted the uncertainty principle as both valid and ultimate, and laws of probability as the basic laws of physics. But he saw that such a view could not possibly be justified if the quantum entities were constituents of an objective universe whose existence and nature had to be, or had been, discovered. He regarded them accordingly as belonging to what he called a 'form of thought', i.e. 'a predetermined form or frame into which the knowledge we acquire observationally is fitted'. The most fundamental of such forms of thought was the "concept of analysis", according to which we regard the whole of experience as describable in terms of parts, and the next form of thought was that by which the parts were reducible to "identical structural units". 'The aim of the analysis employed in physics', he wrote, 'is to resolve the universe into structural units which are *precisely* like one another.'

If this be so, then clearly when you have reached a level in which all the entities are exactly alike, you have no need to descend further. In the macroscopic world, the parts which are generally recognised as such are obviously not all alike. The variations are considerably reduced when you go to the molecular level, and still more when the molecules are reduced to atoms, but, even so, there are still a hundred or so different types of atom. In the level of quantum particles the diversity is still further reduced, though not completely disposed of, and the variety has increased since Eddington wrote. But he held that this variety was due, to an extent not yet recognised, to relations between the fundamental particles rather than to the the nature of the particles themselves. 'In current quantum theory', he wrote, 'the analysis approaches, but has not yet reached, this ideal. For that reason quantum physicists are still unsatisfied that they have got to the bottom of the relationships of the various kinds of particle that they recognise, and of the connection between gravitation, electromagnetism and quantisation. For my own part I think that the account given in most books on quantum theory by no means represents the full extent of our present knowledge of this problem. If more attention is paid to the relativity side of the problem, the main outline of the extension of physical theory from the present halting-point of quantum theory to the ultimate structural units is fairly clear.'

Eddington's account of this extension has not met with general acceptance, but for our present purpose this is immaterial. The point is that, according to this view, when you have reached a level in which all that goes on is expressible in terms of relations between identical units, you have reached finality—a finality imposed not by the nature of the objective world but by that of the thought-form which you adopt for describing the facts of experience. I think that the conviction of the orthodox quantum physicists that the basic level has now been reached arises from an intuitive acceptance of this view, notwithstanding that many of them, at least, repudiate it in favour of the alternative view that they have discovered an objectively existing foundation of the universe. So long as they maintain that alternative view, it seems to me that they cannot escape Bohm's criticism. But if, on the other hand, they are willing to acknowledge all levels below the macroscopic domain as conceptual, then there is nothing in the least unscientific in their claim which Bohm challenges.

But I repeat that all this has nothing whatever to do with the question whether the present formulation of quantum theory is final or even valid at all. That can be settled only by macroscopic observations of the standard kind, and it may well be that Bohm's sub-quantum level will be found necessary. All that our analysis shows is that a final level is possible, and that the operation in it of laws of probability would be an indication neither that events in the microscopic world were undetermined nor that further analysis need be undertaken to reduce such laws to causal laws. Indeed, as I hope to show later, if one resolves to describe the world in terms of the interaction of multitudes of identical units, he is almost inevitably committed to the use of the mathematical theory of probability because that is precisely the mathematical theory that shows the scope of such interactions. But he must not then impose a picturesque symbolical representation of that theory on the world of experience.

I TURN now to another aspect of Bohm's book—namely, the relation between the causal and statistical regularities which appear in sequences of events. I use these terms advisedly, in preference to 'causal law' and 'law of chance', because I think they more directly, and more accurately, represent the real problem which Bohm, like the rest of us, is trying to face. By a causal *regularity*, or causal *sequence*, of events I mean such a sequence as throwing a stone at a window and the window breaking, and by a statistical regularity such a fact as the occurrence of approximately equal numbers of heads and tails in any large number of tosses of a normal coin. These are direct facts of observation, which agree in allowing us to predict with confidence that if we do the same thing again we shall get a similar result: they differ in that the first is a single, unanalysable occurrence, while the second is necessarily composed of a large number of similar occurrences, the outcome of each of which is quite immaterial to the final result. Causal and statistical *law*, on the other hand, are represented by, say, Newton's law of gravitation, $F = GMm/r^2$, and the Boltzmann expression for entropy, $S = k\cdot \log W$. These, in their general form, do not represent any sequence of events at all. The law of gravitation is supposed to hold good as well for a body resting on a table as for one falling to the ground, and the law of entropy has the same independence of anything that may happen to occur. For that reason I shall consider particular events that are said to exemplify the general laws rather than the laws themselves.

Let me begin by referring to the two types of mechanism to which Bohm refers, namely, deterministic and indeterministic mechanism (see p. 226). He distinguishes these by defining the first as the belief that statistical regularity is ultimately reducible to causal regularity, and the second as the belief that causal is ultimately reducible to statistical regularity. The definitions may be valid enough, but if so I suggest that, in conceiving them, we overlook the fact that there is not the slightest reason to suppose that either regularity is reducible to the other, and that fact gives the whole problem a different character.

To begin with, let us restrict ourselves to the macroscopic level in which, as we have seen, all observation takes place, and I will take first an example outside physics, for greater generality. In physics the observations are all records of measuring instruments, but both causal and statistical regularities are evident in a far wider range of observation than this. Let us consider, then, what I have been told is a fact—namely, that Post Office officials can predict, with good accuracy, the proportion of envelopes that will be posted in any given year without bearing an address.

Now here, each such posting, if examined, would, it is fair to assume, be explicable in terms of circumstances quite independent of those attending other postings. For example, when a series of circulars is being addressed, one may become stuck to the bottom of another. In a second case, the writer may be called to the telephone immediately after sealing the letter, and the maid may heedlessly sweep it up with the others and take it to the post. Now all our experience justifies us in supposing that each of these events can be traced back from cause to cause without our encountering anything that would have resulted in some other envelope having been posted unaddressed if it had not occurred. A child put his sticky finger on one envelope; he did so to climb on the table; he climbed on the table the better to see something through the window; and so on. None of this succession of events, we are bound to believe, could have prevented one other envelope, and no more, from not bearing an address during the year in question. Thus, every such occurrence can be accounted for in causal terms alone. Extending this over the whole field of such operations, we may say that, with negligible exceptions, we could account satisfactorily for every posting in purely causal and independent terms; and therefore, within this field of experience, causal regularity is sufficient to explain everything that happens.

But now, what of the statistical result? That is something superposed on the causal and, so far as we can see, not dependent on it at all. The fact that, say, the unaddressed envelopes are regularly $1/n$ of the total number, is something quite additional to the knowledge we may suppose we have of the causes leading to each individual case. If we had never thought of making such a statistical investigation we should have said that we had a complete causal explanation of all that had occurred. But when we do make that investigation we discover an additional fact. It does not in the least alter anything we have concluded about the causal sequences, but is a piece of quite additional knowledge, not qualifying but supplementing the knowledge we previously had, which knowledge nevertheless we could justly have held to give a complete explanation of all our observations.

We have, then, two types of regularity, both exhibited in the same set of observations and yet quite independent of one another. We cannot even begin to express one in terms of the other without destroying them. If we suppose that a particular posting is affected at all by the necessity of preserving the statistical relation, we deny the sufficiency of the sequence of causes already established as having brought it about. If, on the other hand, we suppose that the statistical relation is a necessary consequence of the cooperation of the various sequences of causes, then we must suppose that those sequences are related to one another in a way which their very nature shows not to be possible. There is no alternative, it seems to me, to the admission that we have here two essentially different kinds of regularity, one applicable to single events and the other to collections of similar events, and the expectation that we can ever explain the single event in terms of the properties of the assembly, or the assembly in terms of the properties of its individual constituents, must be abandoned as a delusion.

The fact that the causal type of explanation seems automatically to satisfy our desire for understanding, while the statistical type leaves us with a feeling of something in the background not yet brought to light, must, I think, be an effect of familiarity. From our earliest days we are accustomed to seeing a ball move if we kick it or a cup fall if we let it go, but we are not so attentive to the fact that after a thousand miscellaneous kicks the ball is still not very far from its original position. But in fact, apart from experience, we could no more deduce that the cup would fall than that it would rise or do nothing at all or do different things on different occasions. The fact that there are *causal* regularities is thrust on our attention involuntarily, while the fact that there are *statistical* regularities has to be discovered in comparatively rare cases by *ad hoc* research; but fundamentally both types of regularity are on precisely the same footing, as aspects of our experience which we have to accept but which it is futile to expect to understand in the sense of seeing that it must be as it is. Accepting the fact that regularities exist, we can try to discover them, but to ask why they exist is as profitless in the one case as in the other.

If this be so, then I think we can see how some, at any rate, of the baffling questions concerning "chance" or "indeterminacy" in nature have arisen. They are due largely, I think, to the unconscious but false assumption that the mathematical theory of probability is a necessary tool for the analysis of statistical law. There is, of course, no question that it is a possible tool, and it is equally undeniable that in many problems it yields valuable and valid results which, at present at any rate, we should not have reached without its use. But what I think has been overlooked is that its application to the problem of statistical regularity pre-supposes that such regularity must be expressible in terms of causal regularity—or, more generally, that a property of an assembly must be expressible in terms of a property of each of its units—and this error, I think, is responsible for much of our perplexity.

For example, we find that in a large number of throws of a normal die, the number 4 comes uppermost in about 1/6 of the cases. That is a fact about the whole collection of throws. But we express this by saying that each throw has a probability of 1/6 that it will show 4; i.e. we transform a property of the collection into a hypothetical property (probability) of each throw. In the example I chose just now we would say that the probability of a particular envelope being posted unaddressed was $1/n$, as though each act of posting had some property belonging to it which revealed itself only when that act was considered in relation to a large number of others. However unexceptionable this might be as a mathematical device, it is meaningless as a literal statement. The single event has no such property, for it would be exactly what it is if no other such events occurred.

How dangerous this form of expression is may be seen by its almost automatic introduction into these considerations of the word 'chance', with its connotation of caprice which the facts not only do not suggest but doubly exclude. If we had never thought of making the statistical inquiry we should have regarded the causal regularities as completely explaining the whole situation. When we do make the statistical inquiry we find another regularity, so our knowledge is increased; but we express this in a form which indicates that it has *decreased*: the determinacy which is discovered in the behaviour of the assembly is transformed into an indeterminacy in the behaviour of the individual, whose behaviour had nevertheless already been shown to be fully determined, and so an addition to knowledge is distorted by our voluntary act into a loss of knowledge. Thus the door has been opened to a mass of speculation about the fundamental characteristics of nature which in fact are characteristics of our mathematical machinery alone.

Let us, however, proceed. Still keeping to the macroscopic level, let us now turn from everyday events to events of the kind dealt with in physics. These, as we have seen, are characterised by two qualities which distinguish them from such operations as posting letters. First, they would never have happened if we had not arranged to perform them, according to strictly specified instructions, for the purpose of deriving physical laws; and secondly, they all result in readings of measuring scales of instruments, and these readings alone are regarded as significant. The consequence is that statistical regularities, such as the definite fraction of unaddressed envelopes or of throws of 4 with a die, do not arise, because we choose our operations so that they are eliminated. All our measurements are such that repetition gives, as nearly as possible, the same result. Thus, we do not measure lengths with a rubber rod or a growing worm, but with a specially prepared rod having certain properties which we regard as essential for the purpose. There is nothing whatever in nature to assure us that the relation of a given distance to such a rod is more significant than its relation to a worm, but we pay attention to the former, and ignore the latter, because then the *statistical* calculation associated with many measurements is without significance. The result is that we ultimately obtain a system of causal law which we can apply to each individual case with the same degree of satisfaction as that attending our account of the causes leading to the posting of a single unaddressed letter; and this time, in the vast majority of cases, there is nothing left over to perplex us.

For example, if we drop a stone from a tall tower many times, the points of impact on the ground will generally be scattered, as it seems, irregularly. In the pattern thus formed we select a particular point, which we call the centre of position, which is such that the deviations from it of the actual points cancel out, and we regard this point as having special significance. Further falls do not change it appreciably. We have thus obtained something free from statistical disturbances, and therefore we can regard it as an effect of causal influences alone. We find, in fact, that the point in question is the point on which our causal laws of motion, applied to a rotating Earth, would require the stone to impinge. We therefore say that we have verified the causal law. But, strictly speaking, that law has been falsified in every case, because in none of the falls did the stone strike that particular spot. Although in each case we have done our best to eliminate everything that we regard as a disturbing influence, and no such influence is perceptible to us, nevertheless each fall occurs as though either there were a disturbance or else the causal law were inaccurate. We do not hesitate to choose the former alternative, and feel quite satisfied that *if* we had observed accurately enough we should have detected a causal disturbing influence which would have made each individual fall completely describable in purely causal terms.

Nevertheless, there is a problem left over which we choose to ignore. How is it that all these disturbing influences, each independent of all the others, happen to cancel out? It is the same problem as our former one: how is it that the number of unaddressed envelopes is $1/n$ of the total number? The fact that what we have to explain bears the number 0 instead of $1/n$ is epistemologically negligible: possibly if envelopes bearing two addresses had been counted as negative, the number would have been zero in that case also, but that would not have annihilated the problem. But because in this case we deliberately choose operations in which the number is zero, we feel a sort of spurious justification for ignoring the statistical regularity, and, for a long time at least, we regarded causal law as having universal and undivided authority over the operations of nature. Actually, what we had established was that the effects of causal law appear when those of statistical law happen to neutralise one another, and by carefully restricting our considerations to cases in which that condition held, we claimed the right to declare causal law supreme.

This appears clearly enough if we suppose that we had had no reason to suspect that the Earth was rotating. In that case, causal law would have led us to expect a centre of position vertically below the point of release of the stone: in fact, it would have been to the east of that point. To be consistent we should then still have said that causal law operated in every fall, but that in addition there was a statistical law which gave a centre of position differing from that indicated by causal law alone. But in fact we should have done nothing of the kind. Keeping to our prejudice that in all physical operations the effects of statistical law must cancel out, we should have said that our deduction from causal law was wrong, and we might even have "discovered" the rotation of the Earth from these observations.

So far as I know, there is only one phenomenon in the macroscopic level in which this device for ignoring statistical law appears to fail, and that is the Brownian movement. Here you follow a prescription as closely as possible—dropping similar small spores in a homogeneous liquid and keeping the temperature and other conditions constant—and you expect the requirements of well-established causal law to be observed: each particle should rest suspended or fall smoothly to the bottom or rise smoothly to the top of the liquid, depending on the relative densities. Actually, nothing of the kind occurs. Each particle moves about, apparently quite irregularly, just as though it were constantly being bombarded by randomly moving objects. This continues however long you leave the mixture: a steady state is reached for the system as a whole, but never for the single particle; that goes on moving about irregularly, as long as you care to watch it.

Now here the supposed disturbing influences, instead of slightly changing the behaviour prescribed by causal law, dominate the scene, and the causal regularity is not shown by a single particle. The apparently lawless agitation completely masks the tendency, if any, of the particle to rise or fall. What we do to cope with this situation is interesting. We assume the existence of invisible particles, moving about in such a way that when one of them hits a visible particle, the latter responds exactly as though there were an impact with a material particle obeying the causal laws of mechanics. If the visible particle moves to the right, the impact has come from the left; the greater the change of motion of the visible particle, the greater the momentum of the bombarding particle; energy is interchanged between the visible and invisible particles according to the law of equipartition; and so on. In other words, the invisible particles are of exactly the same mechanical character as the visible ones, obeying exactly the same laws, the only significant difference between them being that one set is observed and the other not.

The equivalent of this in our example of the envelopes would be the hypothesis of the (presumably unobserved) sticky finger, and the part it plays is that of enabling us to maintain a causal sequence for the behaviour of each individual particle. We make the matter very easy for ourselves by supposing the same kind of cause for every irregular movement—a most improbable occurrence with the envelopes—but otherwise the two cases are quite parallel. All this has no bearing at all on the general distribution of the whole set of spores in the liquid—i.e. on that which corresponds to the existence of one unaddressed envelope in every *n* posted. With that we do not concern ourselves. There is nothing whatever in the fact that each spore is knocked about at random to ensure that, on the large scale, the spores shall be regularly distributed in the body of the liquid, as in fact they are. The element of the falling stone experiment that is impossible here is the ignoring of the disturbing influence that affects each individual fall; but the statistical regularity that is manifested in both experiments is ignored in both experiments.

For that reason the Brownian movement throws no light at all on statistical law, and, so far from accepting it as evidence of anything of the nature of 'chance' in the macroscopic world, we deliberately invent invisible molecules in order to *exclude* chance, i.e. to maintain our belief that every motion and every change of motion of every spore is the inevitable result of a proximate cause, differing in no essential respect from the movement of a football during a match if we suppose the players invisible. By the same token there can be no indeterminacy in the motion of the hypothetical molecules that hit the spores, for if there were we should have gained nothing. If we are allowed to release the molecules from obedience to strict causal law, we might as well have released the spores themselves: we postulate the molecules for *the sole purpose* of avoiding indeterminacy. On the whole, therefore, the Brownian movement introduces nothing new into our considerations.

This completes our discussion of the macroscopic level—in which, I repeat, all observation occurs. Two types of regularity appear, which seem to be quite independent of one another. The observations dealt with in physics are purposely chosen so that the effects of one type cancel one another, thus making it possible to ignore them and to suppose that only the other, the causal type, exists in nature. In the one case in which this device fails, we postulate the operation of entities outside the macroscopic domain, for the sole purpose of maintaining the universal applicability of causal law to individual cases.

It would take us too long to examine in detail the lower levels step by step, nor is it necessary to do so for our main purpose, which is to understand the precise significance of the contents of those levels in relation to our actual experience. For that reason it is not important to us how many such levels there may be, what entities exist in any of them, or how such entities are supposed to behave. Their one all-important characteristic, in which they differ intrinsically from the macroscopic level, is that whereas in the latter whatever hypotheses we make can be directly tested by observation of the entities themselves, no hypothesis concerning the entities of the lower levels can be tested by observation of those entities but must stand or fall by its implications for macroscopic entities, of which alone observation can be made. The consequence is that, while on the one hand the microscopic entities have the disadvantage of not being observable, they have the more than compensating privilege of being able to assume any properties at all (that are not in logical contradiction) so long as the ultimate implication of those properties for the domain where observation is possible are in fact consistent with observation.

Let me take an example. In the Sherlock Holmes story, *The Sign of Four*, the dog Toby follows the scent of creosote through the streets of London. At one point the scent divides. He first follows a path that leads to a destination in which the great detective is not interested, so he is brought back and allowed to follow the other path, this time with satisfactory results.

Now here, so far as observation is concerned, we have the precise equivalent of a Wilson chamber experiment. Toby uses his nose and the physicist his eyes, but they both observe a track which at one point divides into two, and they both regard each track as an indication that previously some entity has followed a course standing in a one-to-one correspondence with it.[1] But now, there is a striking difference in the inferences drawn. Sherlock Holmes deduces that at the point where the fork occurs, another sample of creosote has crossed the path of the important one. It is the trail of this interfering sample that Toby follows first, and then, that having been dismissed, he follows the second, and Holmes has complete confidence that that is the unchanged continuation of the original trail. The physicist, however, deduces that the entity responsible for the original trail went out of existence at the fork, and that two other entities concomitantly came into being and pursued divergent paths. This appears to him to be the only tenable explanation of his observations. How is it, we can imagine him asking in wonder, that so great an intellect as that of Holmes should have overlooked this obvious possibility? The fact that Holmes's hazardous guess nevertheless turned out to be the truth the physicist can only ascribe to the circumstance that he belongs to a story and not to real life.

But the truth is that, although the phenomena are similar, the conditions to be satisfied by the explanations are quite different. Holmes is operating in the macroscopic domain, and the physicist in one of the lower domains. Holmes had already decided that the trail he was following was that of a man who had stepped in creosote, and he was not free to suppose that this man had suddenly become twins because we can observe men and we know that that is a sort of thing they do not do. The physicist, on the other hand, had decided that the original trail was that of, say, a nitrogen nucleus, but he did not know, and could not know, that there was anything at all that a nitrogen nucleus might not do, for he could not conceivably observe it. All he has to account for is the track on the photographic plate—consistently, of course, with his explanations of other physical observations—but Holmes has not only to account for Toby's behaviour; he has to find his man. It never occurs to the physicist to seek out the particles he postulates, to see if he is right, for he knows that to be impossible. But what he can ignore is for Holmes a categorical imperative.

But the physicist abuses his freedom when he tries to impose the characteristics of his inventions on the entities in the observable world. This abuse is exemplified most commonly in the assumption, akin to one already discussed, that because we can give a satisfactory account of what we observe in terms of entities to which we assign *a priori* probabilities, therefore each *observable* event has a definite probability of occurring which falls short of certainty by an assignable amount. *A priori* probabilities of individual behaviour are permissible in our microscopic scheme, for the only condition which that scheme has to satisfy is that it shall not finally issue in anything contrary to observation; there is no reason at all for expecting that its details shall be duplicated in the observable world. Furthermore, we have no right to suppose that there may not be an indefinitely large number of such microscopic schemes, all differing from one another, which may yet be consistent with all observations so far made. The orthodox quantum scheme and one on the lines of Bohm's suggestions may not unfairly be cited as possibly two such schemes. Supposing them free from logical errors, the only way of deciding between them is by macroscopic observations. If in those they agree in every particular, then the choice can depend only on aesthetic considerations. But in order to prove that any such scheme is final, it would be necessary to show that it must agree with all possible observations and that no other scheme can do so.

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[1] For a sample of many such Wilson chamber tracks, see a photograph by N. Feather, *Proc. Roy. Soc.* A, Vol. 136, Plate 16, No. 18 (1932).

We can see at once that such a proof is impossible. No distinction can be made by observation, within a finite time, between a scheme that requires observed events to happen with certainty and one which requires them to happen with a probability less than, but indefinitely close to, unity. Even when the probability is definitely assigned, but is yet so near unity as that which modern quantum theory requires, the time needed for a decision would be far too long (apart from a lucky occurrence, of which more presently) for the question to be of any practical interest. The view that all predictions of observations have a definite probability, less than unity, of being realised therefore cannot be disproved.

Nevertheless, I doubt whether, in his heart of hearts, anyone believes it. It implies that, in Bohm's words, "***in a long enough time*** or ***in a large enough aggregate***, every possible combination of events or objects will eventually occur", or, as I think Jeans put it, if we continue for an indefinite time to put kettles of water on the fire, it is certain that the water in one of them will freeze. Actually, I suppose, 'certain' here ought to be 'practically certain', but, on the other hand, it could be added that this event is just as likely to occur with the next kettle of water as with one in the remote future. My own conviction is that it would never occur, and while of course I cannot prove this, I should look with some curiosity at anyone who assured me that he was convinced that it would. Eddington (1935), notwithstanding his view that we can be certain only of probabilities, admitted frankly that if he should ever experience such an event he would think it more probable that the scheme which required it was wrong than that it had proved itself right.

But if this is so—if, in fact, we have devised a microscopic scheme which can justify itself only by making it almost impossible for us to discover its failures—is that not a sure sign that it is but a device for enabling us to predict what we shall observe in given circumstances, and nothing more? If we have a practical problem that leads to a cubic equation having one real and two imaginary roots, and we find that, in fact, the real root does agree with the observational solution of the problem, we do not feel compelled to say that the physical situation must contain something corresponding to the imaginary roots. We regard them as relevant only to the mathematical machinery employed, which happens to be applicable to more general situations than that actually before us. When we suppose that the probabilities assigned to the fundamental particles are carried over into observable characteristics of macroscopic bodies, we are doing just what we should be doing if we gave physical meaning, relevant to our problem, to the imaginary roots.

But I think we can see why such probabilities are exceedingly likely to arise in physical theory, if we remember that we begin to construct our conceptual scheme by postulating the existence of discrete entities which we then proceed to analyse down to basically similar units, in accordance with what Eddington called a 'frame of thought'. Our first set of such entities—the "molecules" which cause the Brownian movement—are clearly not all alike—those in different liquids cause different movements, for example—so we proceed to reduce them in the familiar way, first into atoms, then into electrons and such particles, and so on. At each stage we approach nearer to a universal similarity, but so long as we define each entity by its actual behaviour, we do not succeed in reaching such similarity. For example, we can account for the chemical behaviour of hydrogen by supposing that all its atoms are alike, but when we consider their radiation we find that, in the same external circumstances, some emit Hα, some Hβ, some Hγ, and so on. We therefore analyse the atom still further, into a proton and an electron, and then the retrograde step of substituting two particles for one is far more than compensated for by the fact that these same two particles, together with a third, the neutron, can account for the hundred or so different kinds of atom that we had previously had to acknowledge as fundamental. But still we have not reached our goal because if, in the same circumstances, one electron makes the transition that radiates Hα, and another the transition that radiates Hβ there must be some difference between them. Clearly there is no guarantee that we shall ever reach finality if we regard difference of behaviour in identical circumstances as a criterion of difference of character, but if we define the electron in terms not of its behaviour but of its *probabilities* of behaviour, then we can say that all electrons are exactly alike because they have the same set of probabilities of making the whole set of possible transitions. The fact that different electrons make different transitions merely exemplifies the identity of their probabilities.

This assignment of probabilities as a defining property of an electron is, of course, perfectly legitimate because an electron is a conceptual entity and we can conceive it as we like. This is quite a different case from the assignment of a probability to a single throw of a die, which, being observable, can have only potentially observable properties, and there the relevant observation is a property of a large number of throws, not of a single one.

The difference, then, between Bohm and the orthodox quantum physicist is that the latter thinks the time has come to take this step and the former prefers to go on analysing into still deeper levels, with the possibility, but not certainty, of at last arriving at one in which the entities are alike in their actual behaviour. From my point of view it is a purely practical question which is to be preferred: that which gives the more satisfactory results in the field of observation will eventually triumph. But in so far as they both agree in supposing that all levels have the same fundamental character, as equivalent constituents of a wholly objective physical world, I say, 'a plague o' both your houses'. Such a supposition is directly at variance with the actual way in which the constituents of the levels have come to appear in physics, and with the fact that observation of the macroscopic entities is possible and that of the others is not.

We can now see even more clearly than before, I think, how incongruous are the supposed experiments exemplifying the uncertainty principle by the process of observing an electron. We observe a macroscopic body, and to explain this we postulate something called *light*, which conveys knowledge of the body to the eye. Light itself, of course, is essentially invisible; otherwise there would have to be a second kind of light by which to see it. If it could be visible without this second kind of light, then the macroscopic body itself could be so visible and the first kind would be unnecessary. Light, therefore, belongs essentially to the lower, purely conceptual levels, in which it becomes describable as photons. In those levels it has precisely the same epistemological character as the electron, the proton and the others, all of which have the same intrinsic freedom from possibility of observation. To try to picture the process of observing an electron, therefore, is to destroy its whole character and mode of being, by forcing it into a level to which it cannot belong. Thought-experiments are legitimate if they could be, even though they have not been, carried out, but thought-experiments which are impossible by the very nature of the entities concerned in them stand self-condemned. They are as legitimate as the conclusion of the nineteenth-century atheist, who denied the existence of God because he could not observe him in his microscope.

To sum up, then, we find by observation that two kinds of regularity—causal and statistical—occur in nature (i.e. among observable things), one characterising single events and the other collections of events. They are both exhibited simultaneously in the same set of occurrences, and it would appear impossible to express one in terms of the other so long as a single event can be distinguished from a set. The domains of microscopic physical theory, lying below the macroscopic domain in which alone observation is possible, possess only conceptual characteristics, which may be assigned with complete freedom so long as there is no logical contradiction and they do not lead to expectations of observable events which are at variance with observation. Because of the essential difference between the macroscopic and the lower levels, it is illegitimate to impose on the former the qualities which may be found useful in the latter, and to draw conclusions from the imaginary process of observing what by its nature is unobservable.

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Inscriptionalism and the Objects of Explanation

by SAMUEL GOROVITZ

1 I WISH TO CONSIDER the question of what sort of thing it is that an explanation explains, and some ontological issues relating to that question.

Preanalytically, it seems safe to say that it is events that are explained, at least in a significant proportion of instances. But I know of no acceptable account of what sort of entity an event is. I assume that events involve physical objects in one way or another, that they are particulars, and that they are locatable in space and time—though I do not pretend to know precisely how, nor what criteria of identity are appropriate for events.

Whatever events are, when we refer to them we do so by using some description of what happened. We may claim, for example, that Johnny broke the lamp, or we may claim that Harry's son destroyed an antique. Yet, Johnny's breaking of the lamp may be Harry's son's destroying an antique. The two claims differ in that we refer to the event in question under different descriptions.

This distinction between events and their descriptions is of importance for the investigation of the structure of explanation. As an example of the confusion which can result from failure to observe this distinction, consider the unpredicted death of Jones, where P is the description 'the unpredicted death of Jones', and Q is the description 'the death of Jones'. It seems clear that prior to the event in question it was in principle not predictable under description P. For to attempt to predict it under that description would be to render the description no longer applicable. This argument however, in no way tends to show that the event in question was unpredictable. For, unpredictable though it was under description P, it may well have been predictable under description Q. That it in principle was would follow from some deterministic views of the universe. But the lack of predictability under P ought not to be taken as an argument against determinism. To take it so results from failure to distinguish between the predictability of events and the predictability of events under particular descriptions.

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I have been aided in revising this article by criticisms raised in colloquia at Washington University, Stanford University, and Macalester and Hamlin Colleges. In particular, Professor Carl Wellman's remarks prompted the removal of several flaws.

What is recognised as an acceptable explanation of an event under a particular description ought not to be mistaken for an explanation of the event under other descriptions. To explain an event under *P* (where *P* is an event description) may not be to explain that event under *Q* (where *Q* is another description of the same event).

2 I NOW EXAMINE some ontological issues related to speaking about events under descriptions as the objects of explanation, and in this connection, shall consider the theory of inscriptionalism as developed by Scheffler (1954; 1963, pp. 57–76) and discussed by Quine (1960, §44).

Scheffler is aware of the issues considered thus far, and he clearly shows how speaking of the explanation of events, pure and simple, will lead to contradiction. He concludes that "we cannot construe explanation as a relationship between explanatory premises and concrete events". Rather, when we provide an explanation, we must not take ourselves "as having explained some concrete spatio-temporal entity *e*, but, at best, as having explained *e*-as-described-by-*P*, or the fact that *e* is truly described by *P*". But, by existential generalisation, if we have explained an event-as-described-by-*P*, or a fact, then there must *be* an event-as-described-by-*P*, or a fact that we have explained. The ontological consequences are clear. We must now recognise the existence of these "new entities", which are "abstract ('logically intensional') entities, intermediate between" the concrete events that we cannot explain and the descriptions by means of which we characterise them (Scheffler (1963) p. 59). For Scheffler, admitting the existence of facts constitutes an unacceptable departure from "robust materialism", and he thus addresses himself anew to the question of the nature of the objects of explanation. (The phrase is Scheffler's; that there is anything robust about materialism is a tenous claim.)

What Scheffler seeks, clearly, is an account of what it is that gets explained, such that no entities are presupposed to exist by that account except such entities as are acceptable to the nominalistic position he defends. What is not so clear is whether facts are offensive merely in virtue of being abstract, or in virtue of being "logically intensional". Scheffler nowhere indicates either what it means to say of an entity that it is abstract or what it means to say that it is intensional, nor is it clear from his discussion even that he takes abstractness and intensionality to be different failings. Scheffler credits the use of the term 'logically intensional' to a suggestion of Quine, who writes that intensional objects are "only objects *x* such that any two conditions uniquely determining *x* are analytically equivalent" (Quine (1961) p. 152). Later, he writes more specifically that such objects, if such there be, include "monadic intensions, or attributes, and dyadic intensions, or relations", as well as "medadic (o-adic) intensions, or propositions" (Quine (1960) p. 165). Since identity of membership entails identity of classes but not analytic equivalence of determining conditions, classes are not intensional objects. Yet they are abstract entities, and thus intensionality and abstractness are different characteristics. Of course, intensional entities are all abstract, and thus the elimination of intensional objects from the ontology represents a more conservative asceticism than the elimination of abstract entities.

Scheffler's method of avoiding unwanted ontological commitment in the analysis of explanation is to construe the objects of explanation as sentences, and further to construe "sentences themselves, generally, as ***inscriptions*** (physical objects of certain shapes), rather than as abstract shapes" (Scheffler (1963) p. 74). As example, he cites the production of a certain blue flame that may be described either as 'the flame in the chemistry laboratory of University *X* on 8 January 1907' or as 'the first flame produced by application of Frankenstein's method'. Let '*g*' designate sentences that explain why the first flame produced by application of Frankenstein's method was blue. We cannot infer that *g* explains why the flame in the chemistry laboratory of University *X* on 8 January 1907 was blue, of course, even though there is but a single flame in question. Scheffler proposes that we take explanation "as relating sentences to *other sentences*" and, thus, in the case at hand, we can say that the sentences *g* stand in the explanatory relation to some sentence, call it '*Bk*', to the effect that the first flame produced by application of Frankenstein's method was blue (Scheffler (1963) p. 72). There is thus no need to speak of facts or other logically intensional entities as objects of explanation.

Two observations may be made at this point. First, such an analysis depends on the legitimacy of construing sentences as physical objects, in particular, sentence-tokens (inscriptions) rather than sentence-types. I shall return shortly to this issue. The second observation concerns the relation that holds between *g* and the sentence explained. It seems that Scheffler is committed to the existence of the relation that he claims holds between *g* and '*Bk*', and this commitment would seem unacceptable, unless he can provide an analysis of that relation which construes it as extensional and indeed as concrete. Perhaps an analysis in terms of the notion of ordered pairs would remove the intensionality. It is hard to see how the abstractness can be avoided. Scheffler pursues this issue no further, as if sweeping the intensionality under the relational rug eliminates it altogether.

But this last observation does not undercut Scheffler's inscriptionalism, since his suggestion that explanation be construed as a relation between sentences is only offered as an alternative strategy for avoiding commitment to intensional objects. His primary thesis (where '*Bk*' is the sentence to be explained, and '*EF*' designates the "explains the fact that" operator) is that

> We acknowledge '... *EF*——' simply as an *operator*, forming sentences by connecting names and other sentences.... Thus, for example,
>
> *gEF*(*Bk*)
>
> is to be taken as consisting of the name '*g*' and the one-place predicate '*EF*(*Bk*)' formed by inserting '*Bk*' into the second blank of '... *EF*——'. The whole statement thus applies a one-place predicate to the sentence string *g*. It does not relate *g* to anything else. But, of course, the statement is true if and only if *g* fulfills the criteria of explanation with reference to '*Bk*' (1963, p. 71).

The next step is the construal of the sentence '*Bk*' as a concrete inscription, along with the stipulation that when a sentence inscription is explained, so too are all logically equivalent sentences. We need not worry whether an appropriate inscription exists as the object of any given explanation, for "merely to formulate the question why *k* is blue, for example, is to produce an inscription of the right sort, namely, the '*k* is blue'-inscription used in putting the question" (1963, p. 74).

I grant the legitimacy of Scheffler's assumption that the question involved is written when there is such a question—e.g. when the explanation has been offered in response to a request for an account of why *k* is blue.[1] Yet, it seems easy enough to imagine cases where no appropriate inscription is produced in such a way, because no question is asked. Imagine a lad who, having seemingly misbehaved, offers an unrequested explanation of why he did what he did in the hope of thereby providing justification. No question need be asked—and, hence, no appropriate inscription need be produced; unless it can be shown, as Scheffler believes, that to provide explanation is necessarily to include, in what one provides, an inscription of the required sort. But this belief is not warranted, as we shall see below. Or suppose I reflect on a puzzling event, and, seeing connections I had not seen before, arrive at an explanation myself without having spoken or written a word. Would we be willing to say that one cannot explain to oneself in this way that involves thought, but no speaking or writing? Or would be allow that in such cases the requisite inscription must somehow exist? Either option seems implausible.

Now let us approach the question of inscriptionalism from a slightly different tack. Scheffler's inscriptionalism is called upon to do multiple duty. If

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[1] This assumption simplifies, but does not prejudice, the issue.

(12) John desires that John qualify for entrance to medical school, we can ask just what it is that John desires. As inventiveness lags, ontology bloats. If the object of John's desire is a future state of affairs, we must acknowledge that it is a possible future state of affairs that may never become actual. John may never qualify for medical school; all the same, his desire may be real. If we admit that the objects of desire are possible states, we are as far from robust materialism as those who countenance facts. Here, again, inscriptions come to the fore: "... take ordinary 'desiring that' statements, such as (12), as tantamount to statements expressing a certain relation between agents and inscriptions." (Scheffler (1963) p. 101). We are to reconstrue (12) as

$$(\exists x)(\text{That } (QJ)x \,\&\, DTrJx)$$

which is read (p. 100):

> 'There is some *x* such that *x* is a that-John qualifies-for-entrance-to-medical school, and John desires-true *x*'.

with the range of variables restricted to concrete inscriptions. Thus, in the absence of other appropriate inscriptions, if I assert (12) by writing it, I am claiming a relationship of a specific kind to hold between John and the inscription I thereby produce. But what if no one ever produces an appropriate inscription? Scheffler assures us that we need not fear. "When mentioned in descriptions of desire, belief, and conduct, they [inscriptions] can clearly be trusted to exist ..." (p. 101). John need not himself produce the inscription. It is enough, we are told, that we mention the desire. Scheffler writes (p. 102):

> Also, as noted the 'desires-true' formulation is to be taken as true under just those conditions in which its ordinary 'desires-that' counterpart is considered true. In particular, for an agent to desire-true some given inscription does *not* imply that he produce, possess, wish to possess, be aware of, or even understand the inscription in question.

Of course, for an agent to desire-true some *given* inscription, there must be such an inscription. But the trouble with construing the objects of desire to be inscriptions is that it seems one can have a desire, and hence the desire will have its object, even in the absence of any appropriate inscription.

Consider. John sees a beautiful woman, and looks longingly at her. He does not reflect or comment on his attitude, nor does anyone else. He desires that he meet the woman. But no inscription to that effect is produced. He may be aware that he has such a desire, in the sense that he would or at least could honestly admit to the charge. And if asked of his intentions, he could truthfully say 'I intend to meet that woman'.

The unavoidable fact here is that John has a desire, although no appropriate inscription exists at the time of the desire. Surely the reality of his desire is not dependent on our subsequent description of it or reference to it. If the object of his desire is an inscription, it must be a possible future or abstract inscription that he desires-true. Yet it is hard to see how admitting such objects into the ontology represents any improvement over the states that Scheffler seeks to exclude. Even if inscriptions may consistently be considered as objects of desire when the circumstances provide an appropriate inscription, the possibility of a man having a desire—and hence of there being an object of that desire—in the absence of any appropriate inscription, shows that it is not, in general inscriptions that are the objects of desire. We must either show the notion of the object of a desire to be eliminable (an option favoured by Quine with regard to the propositional attitudes generally), or look elsewhere for characterisation of the objects.

Although inscriptionalism seems to fail in the case of desire, it does not readily follow that it fails for the analysis of explanation. We have yet to examine Scheffler's claim that there will always be, in the case of explanation, an appropriate inscription since we must necessarily provide one in the process of providing an explanation, at least, to someone else. Obviously, no such claim can be made in the case of desire, since the existence of a desire, unlike the provision of such an explanation, does not require any overt linguistic behaviour.

Before we deal with this loose thread, however, let us return to pick up another—the question of the legitimacy of construing sentences in this context as sentence tokens, or inscriptions.

Quine (1960), considering spoken rather than written sentences (but it makes no difference; the issues are the same), says (pp. 191–9):

> A sentence is not an event of utterance, but a universal: a repeatable sound pattern, or repeatedly approximable norm . . . a linguistic form that may be uttered often, once, or never . . . its existence is not compromised by failure of utterance.

Each such linguistic form, he goes on to say, is

> the *sequence*, in a mathematical sense, of its successive characters or phonemes. A sequence $a_1, a_2, \ldots, a_n$ can be explained as the class of the n pairs $\langle a_1, 1\rangle$, $\langle a_2, 2\rangle, \ldots, \langle a_n, n\rangle \ldots$ We can still take each component character a_i as a class of utterance events, there being here no risk of non-utterance

(since, for example, the phonemes of English have all been uttered). Thus, on Quine's construction of sentences, they are equivalence classes of phonemic sequences—abstract but non-intensional; what, on some construals of the phrase, one could call sentence-types, as opposed to tokens. Such a construal of sentences obviously avoids the problem faced by inscriptionalism in connection with the existence of inscriptions as the objects of desire. It has the further advantage, cited by Quine, of providing for an account of existential assertions involving the "propositional attitudes" of believing, desiring, etc. Quine writes (1960, pp. 215–6):

> ... how are we to say, e.g., that Paul believes something that Elmer does not? It will not do to say that Paul believes-true some utterance that Elmer does not believe-true, for it may happen that no such utterance exists or ever will; believing does not, like saying, produce utterances. This defect may be counted as favoring sentences over utterance events as objects of propositional attitudes; for, ... sentences are undisturbed by failure of utterance.

Scheffler, adopting a suggestion of Quine, is willing to retain inscriptionalism by overlooking the inability of his analysis to account for such troublesome existential assertions, agreeing with Quine (1960, p. 215) that they "are expendable; for such quantifications tend to be pretty trivial in what they affirm, and useful only in heralding more tangible information. But Quine's willingness to give up such assertions merely foreshadows his willingness to give up talk about the objects of propositional attitudes altogether (p. 216). Scheffler, on the other hand, denies (1963, p. 109) that "giving up *such* quantifications as are illustrated by Quine's problematic sentences, i.e. isolated existential quantifications, permits us to give up *all* quantification over belief objects . . .". It should come as no surprise that Quine, who in the end wishes to dispense with objects of belief, is willing to give up sentences that assert that two people's beliefs have a common object.[1] The price of the concession for Quine is small. For Scheffler, however, the objection remains. If beliefs and desires have objects, surely we must somehow be able to account for such sentences as assert that two people share a common belief. And Quine has shown, as Scheffler recognises, that inscriptionism will not quite work.

Might we not, then, accept Quine's construal of sentences, and continue to accept Scheffler's view, thus modified, that sentences constitute the objects of belief, desire, and perhaps even explanation? I think not, for reasons suggested by some of the examples Quine himself has provided (though in another context). Suppose that John and I, both knowledgeable in the rudiments of biology, differ in judgment about how interesting our mothers are to us. We agree that in the biological sense, our mothers bore us. But we disagree about whether or not we find them boring. How, then, are we to decide whether or not the sentence 'our mothers bore us' is a shared object of belief for us? On Quine's construction of sentences, phonetically indistinguishable sequences are examples of the same linguistic form or sentence. Of course, if we dispense with the objects of belief, the problem dissolves; otherwise, it remains. We might try distinguishing between the two senses of the sentence in question on the basis of semantic considerations, but, as Quine puts it (1961, p. 58) "treating an ambiguous form as two forms . . . has the drawback of making the concept of form depend on that of synonymy". Such a dependence is notoriously repugnant to Quine, and we can confidently expect that Scheffler would share Quine's discomfort. For, distinguishing forms on the basis of their meanings is to come face to face once again with intensional objects, and as Quine has shown (1961, pp. 20–32), unexplicated reliance on the concept of synonymy fares little better.

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[1] Quine (1968, p. 304), in reply to Davidson (1968), admits that his abandonment of the objects of propositional attitudes had underestimated cost. Davidson's arguments, which differ from mine, first appeared in (1965) and prompted Quine's reply when presented in his (1968).

Sentence types, it seems, provide small advantage over sentence-tokens as the objects of propositional attitudes. But our immediate concern is with the objects of explanation, and it is perhaps unclear at this point how the discussion of the objects of propositional attitudes relates to the original question of what to construe as being the proper objects of explanation. We recall that Scheffler, unwilling because of their intensionality to recognise facts, or events under particular description, as the objects of explanation, sought to give an account of inscription as the objects. Similar motivation led to consideration of inscriptions as objects of the propositional attitudes (believing that, desiring that, etc.). But inscriptionalism ran into hurdles as an account of the objects of desire and belief, and even the rather significant switch from concrete inscriptions to phonemic sequences did not quite do, because of the existence of phonetically indistinguishable sentences that are semantically different (and are free of indicator terms). It is time to look specifically at explanation again.

Part of Scheffler's confidence in inscriptions as objects of explanation was based on his belief that in any context of explanation an appropriate inscription would be provided by the question that called for explanation and also by the explanation itself. I have argued above that it makes sense to speak of explanation in the absence of any such question; it remains to be seen that the provision of an explanation does not include production of the object of that explanation. Consider the following case. A window breaks, and a witness asks a friend, 'What's the explanation?' The friend replies, 'There was a sonic boom'. Here we have a question, elliptical, to be sure, that includes no inscription appropriate as the object of explanation. The reply ostensibly explains. Of course, it does not provide in any sense a complete account of what happened. It presupposes a good deal of knowledge on the part of the inquirer about sonic booms and their connection with windows. So let us grant that the reply fails to meet the criteria of adequacy for explanation. May we not then say that the reply fails, in so far as it does fail, because it does not adequately explain what stands in need of explanation? But the reply contains no inscription of the sort Scheffler seeks as object. Hence, that which stands in need of explanation cannot be an inscription in the reply. Suppose, next, that a third person comes along and provides what we would all accept as a completely adequate explanation. Let us grant that in so doing he produces an inscription of the desired sort. Is it that inscription which he then explains? If so, then what he explains is not that which stood in need of explanation before he came along, since before he came along the inscription he produced did not exist. Further, we are prevented from saying that the first attempt and the final success were aimed at explaining the same thing—that, though one succeeded where the other failed, they were directed at the same object. Yet this seems unacceptably counter-intuitive—there *was* something that both sought to explain, and its existence was not dependent on either attempt. Thus, even on the moot assumption that adequate explanation must incorporate an appropriate inscription, there is reason to believe that the inscription thus incorporated cannot be the object of the explanation.

At this point let us ask again, if inscriptions will not do as objects of explanation, what about sentences as construed by Quine? Here again, there seem to be conclusive objections, because of the possibility of phonetically indiscernible sentences with semantic divergence. Recall the old story about the brothers who inherited a cattle ranch, and named it 'Focus', since that is where the sons raise meat. An explanation of why the sons raise meat will not be an explanation of why the sun's rays meet, yet on Quine's account we deal here with but a single sentence. There is no shortage of such examples, and they do not depend essentially on indexical use of language or referential ambiguity of singular terms. Rather, they show the difficulty of trying to account in an extensional, non-semantic way, for issues of the sort we have been considering.

I do not here deny that problems attend the acceptance of intensional objects. Rather, I hope to have shown that even in light of certain nominalistic efforts to dispel them, the problems of ontological commitment associated with talk about explanation remain.

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The Paradox of Meaning Variance

by JERZY GIEDYMIN

THE PROBLEM whether or not there may be scientific theories which are logically incommensurable with respect to their empirical content and the problem whether or not, as a matter of fact, certain "prima facie conflicting" theories such as Newtonian and relativistic mechanics are so incommensurable, are by no means as new as might appear from recent books, articles and notes concerned with them (e.g. Achinstein (1964); Feyerabend (1965); Fine (1967); Hesse (1968); Scheffler (1967); Smart (1968)). Nor was Logical Positivism as simpleminded and as uniform in its treatment of the relations between alternative theories as is usually assumed by present day critics (e.g. Feyerabend, 1965) and disputants (Leplin, 1969).[1]

A confrontation of the discussion of the problem of comparability of theories thirty-five years ago with its treatment in recent contributions reveals at least the following difference: the older approach was logically far more refined and sophisticated, whereas recent contributions are much richer in historical detail. However, it is clear that the problem of comparability of theories, i.e. the first of the two problems formulated above, is a logical, semantical and not a historical, sociological or psychological problem. If so, no amount of examples or case studies will be sufficient to provide its solution. The second problem of comparability of two specific theories is historical only in the sense that it is a question about two actual, historically given theories. It is not a problem of "historic facts" for which a unique solution might be elicited from historical documents, i.e. relevant texts, by carefully studying them.[2] For in the formulation

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[1] A look at some of the volumes of *Erkenntnis* would suffice to verify this. The problem of comparability of theories was discussed by the author of "radical conventionalism", Kazimierz Ajdukiewicz (1934a, 1934b, 1935). This was recently recalled by R. Suszko (1968) and by J. Giedymin (1968). It is noteworthy that Ajdukiewicz's concept of meaning was pragmatic: meanings, according to him, determined rules of acceptance and the latter determined a language. He believed in the existence of languages which are not intertranslatable and whose extensions are not intertranslatable. It is natural to expect a definite relation between one's concept of meaning and language and one's solution of the paradox of meaning variance. The pragmatic approach to meaning seems to favour the doctrine of incommensurability.

[2] Kuhn (1962, p. 99) writes: 'The man who takes historic fact seriously must suspect that science does not tend toward the ideal that our image of its cumulativeness has suggested . . .' However, it seems to me that the man who takes seriously the doctrine that there are no bare facts but only interpretations in the light of theories, i.e. the doctrine to which Kuhn also subscribes, must not take "historic facts" seriously in the sense that they enforce upon him some unique view of the development of science.

of the question certain terms are used, such as 'the language of a theory', 'the meaning of a term', 'change of the meaning', etc., which are analytic tools of logic, of the philosophy of science or of an interpretative theory of the development of science. There is little hope of solving this problem as long as these analytic concepts are unclear and insufficiently precise. With this in view, I propose to consider here several reformulations or explications of one of the crucial assumptions underlying the problem of comparability as it appears in the form of the so called paradox of meaning variance. In the explications I shall use logical terminology and the concept of meaning postulate. On some of the explications the analysed assumption will turn out to be obviously untenable. The final explication will make the assumption plausible but at the same time will deprive it of paradoxical features so that it will fail to sustain the paradox of meaning variance. In the process of analysing the paradox I shall formulate a condition of its solubility and shall exhibit the role of convention in its solutions.

TO HAVE a convenient point of reference I shall begin by formulating the paradox of meaning variance. I shall do this by fusing two of M. Hesse's formulations (Hesse, 1968a, 1968b) and by adding two further statements:

(1) The meanings of all extralogical terms of an empirical theory are determined by the whole theoretical context and change with change in that context, e.g. 'mass' in Newton's mechanics means something different from 'mass' in relativistic mechanics,

therefore,

(2) given two theories, T_1 and T_2, which are prima facie incompatible, no term P in T_1 has the same meaning as P in T_2,

therefore,

(3) no statement, and in particular no observation statement involving P in T_1 is in a logical relation such as consequence, or contradiction or compatibility, etc., to any statement involving P in T_2,

therefore,

(4) observation cannot decide between such prima facie conflicting theories,

and yet,

(5) scientists do not hesitate to say that experiments turn out in favour of one theory and against another, e.g. Fizeau's experiment in favour of

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Rather he will insist that there are always several reasonable interpretative theories or analytic perspectives possible and the solutions of our historical problems will essentially depend on which of these theories we choose. If the doctrine is correct, it affects the question whether or not science is cumulative as well as whether or not Newton's and relativistic mechanics are commensurable.

relativity and against classical mechanics; Paschen's discovery of the fine structure of the spectrum of helium in favour of the mass increase predicted by special relativity together with Sommerfeld's theory of the atom and against the mass invariance implied by classical physics; or that one theory is a generalisation or extension of another.

(6) If science is empirical, then its growth should depend, among other things, on experimental results as criteria of choice of hypotheses and theories.

Now, WHAT exactly is meant by saying that 'the meanings of all extralogical terms in a theory depend on the whole theoretical context'? What is this "theoretical context" and how does it affect the meanings of scientific terms? Finally, what is the meaning of 'meaning' here?

If we assume that the theory under methodological consideration is in axiomatic form—and every scientific theory is in principle axiomatisable—then, as a first approximation we may explicate (1) as follows:

(7) 1. The meanings of the primitive terms of a theory are determined by the whole theory, i.e. by (*a*) the syntactical formation rules which determine the syntactical category of each expression and define the class of well-formed formulae of the theory, (*b*) by all axioms, (*c*) by the rules of derivation which define the relation of consequence on the class of wff, and so (*d*) by the set of all theorems of the theory, (*e*) by the semantical rules of denotation or direct interpretation. 2. The meanings of the primitive terms change with changes in any of the factors (*a*) through (*e*).

The above reformulation of (1) has the advantage that it clarifies to some extent 'change of theoretical context' as well as 'theoretical context'. Although it does not define 'meaning', it somehow clarifies this concept as well by enumerating factors on which meanings depend. However, the precise nature of this dependence is not yet clear enough. To make it a little clearer we concentrate on (*b*) and (*d*). Presumably we want meanings—whatever they are—to determine the extensions or denotations of the primitive terms. If so, (7) would imply that the denotations of the primitive terms of a theory are determined by all axioms of the theory, which would amount to saying that all the axioms are so-called "implicit definitions" of the primitive terms, i.e. only those assignments of denotations to primitives are permitted under which the axioms remain true. But this would make the theory true *a priori*, i.e. non-empirical. So to avoid this unacceptable consequence of (7) we have to concede that the meanings and with them the denotations of the primitive terms must not depend in this way on "the whole theoretical context" but only on part of it. Some but not all statements of the theory determine the meanings and denotations of its primitive terms.[1] In this way we are forced to accept that in any empirical theory there are two non-empty classes of statements (theorems): the class of statements, which together with certain methods, such as so-called ostensive definitions, determine the meanings and so the denotations of primitive terms and the class of statements which express the theory's empirical content. This is not to say that in the usual axiomatisations of scientific theories the postulates are classified into these two categories or that every axiom performs only one of these two functions, i.e. that of a meaning postulate or that of an empirical hypothesis. The claim made here is that there are methods of reformulating an axiomatised empirical theory in such a way that it consists of two components—the conventional and the empirical—jointly equivalent to the original theory.[2] Every empirical theory may be split into the two components in various ways, which is important for the problem of meaning variance: *the answer to the question whether or not meaning variance occurred in the transition from one theory to another will depend*, among other things, *on how we decide to classify the components of the theory into meaning postulates and empirical postulates*. This decision need not be entirely arbitrary. Normally it is guided by various pragmatical considerations such as the intentions of the authors of the theory, but the latter are usually insufficient to determine the decision uniquely. To that extent the answer to the question about meaning variance or invariance will depend on *convention*. There are, of course, many cognitive problems whose solubility presupposes certain conventional decisions.

Returning now to our explication (7) we conclude that its first conjunct, i.e. (7.1) is acceptable provided clauses (*b*) and (*d*) are suitably modified to allow for the distinction between empirical and meaning postulates. Even so qualified (7.1) seems to go a long way in doing justice to the idea that meanings are determined by "the whole theoretical context". To appreciate this let us suppose[3] that we have an axiomatised empirical theory T with axioms $A_1, \ldots, A_n$ and that this axiomatisation is as close as possible to the textbook formulation of the principles of some non-axiomatised theory. Then it is quite likely that none of the axioms $A_1 \ldots, A_n$ is a pure meaning-postulate, i.e. each of the axioms may have both an A-component (analytic) and an E-component (empirical). Suppose, therefore, that we have re-axiomatised T by $A_{M1}, A_{M2}, \ldots, A_{Mk}, A_{E1},$

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[1] For a similar analysis, cf. M. Przełęcki (1969), especially chapter 4. Also H. Putnam (1965).

[2] That there are methods of distinguishing the A-components and the E-components of a theory was shown by R. Carnap (1963, 1966). Other methods are discussed in Przełęcki (1969) and in Przełęcki and Wójcicki (1969).

[3] I owe the point in the rest of this paragraph to Peter Williams.

. . ., A_{Em} where each A_M is a pure meaning-postulate and the A_E's express the empirical content of T. The meanings of the primitive terms are then determined, in the sense explained before, by the A_M's. But the A_M's themselves may be determined by all the axioms $A_1, \ldots, A_n$ in the sense that any change in just one of $A_1, \ldots A_n$ may result in a change of the meaning-postulates $A_{M1}, \ldots A_{Mk}$, if each $A_1, \ldots A_n$ has an M-component. In that case any change in $A_1, \ldots A_n$ would result in the change of meaning of some terms of T. It does not follow, however, that the meanings of all terms would be affected.

It is clear, therefore, that the qualifications of (*b*) and (*d*) demanded here are perfectly consistent with the view that the meanings of the terms of a theory, as it is normally formulated, depend on the entire theory, that the distinction between the M-postulates and the E-postulates with respect to actual empirical theories is vague[1] and that more often than not every postulate of a scientific theory performs the functions both of an M-postulate and of an E-postulate. On the other hand it would certainly be *irreconcilable with the present analysis to claim that the conventional and the empirical components of a theory are in principle indistinguishable.* It is possible that statement (1) in the formulation of the paradox of meaning variance, or rather the first of its two conjuncts, was intended by some disputants to convey exactly this view of indistinguishability in principle of the conventional or analytic and empirical components of a scientific theory. If, however, this view of indistinguishability in principle were accepted, we should not be able to account for the difference between empirical-scientific and non-empirical mathematical theories in terms of methods of specifying models for them, and—what is relevant for the problem of comparability of theories—*we should not be able to distinguish, either in actual historical cases or in abstract, constructed examples, between changes in science involving meaning variance and changes in empirical hypotheses under which meanings are invariant.* In other words, if the M-postulates and the E-postulates of a theory are in principle indistinguishable, then the problem of whether under certain changes in theories meanings of terms remain constant or not, *is insoluble.* It must be emphasised, however, that this extreme view of indistinguishability is different from the quite plausible view that for the language normally used by scientists the terms 'M-postulate', 'analytic statement', 'synonymous expressions', just as 'simultaneous events' and 'equal distances' in the everyday language, are vague and give rise to essentially insoluble questions unless suitable conventions are adopted. The latter view merely implies that our answer to a question about meaning variance or invariance depends, among other things, on certain conventional decisions.[1]

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[1] Strangely enough many authors believe that by pointing out the vagueness of a distinction they have shown that the distinction is useless for philosophical purposes. Thus vagueness seems to be the standard objection to the analytic-synthetic, observational-theoretical, logical-extralogical distinctions. But in philosophy, as elsewhere, we have to put up with vagueness and use the medicine of convention, whenever necessary.

The extreme view of indistinguishability in principle of M-postulates and E-postulates has certain consequences which have, perhaps, not been fully appreciated. For example, if accepted, it would make inapplicable such methodological concepts as 'falsification', 'falsifiability', 'degree of falsifiability' and 'empirical content' (defined in terms of falsifiability) which play a crucial role in the philosophy of science of K. Popper (1959, 1963) and of his school (e.g. Lakatos, 1968). A basic statement B which falsifies a universal statement S, by definition logically implies the negation of S. If every statement is a meaning-postulate, then—since the negation of a meaning-postulate is self-contradictory—the acceptance of a falsifying basic statement would always commit us to the acceptance of a self-contradictory statement. Alternatively, if it is impossible to tell M-postulates from E-postulates of a theory, then it is in principle impossible to know whether a hypothesis has ben falsified by empirical evidence or a change in the language has occurred. And the whole theory, or indeed the whole of science, would not fare any better in this respect, so that holism would be of no avail either. The same applies to comparability of successive or alternative theories with respect to their empirical content: if there is no meaning-invariance of basic statements, then no two theories are comparable on this basis; if we cannot control meaning-invariance—and we cannot, if the conventional and the empirical components of theories are in principle indistinguishable—then we can never know whether two theories are comparable with respect to their empirical content.[2]

Feyerabend (1965a, 1965b) thinks otherwise. However, his methods of comparing and evaluating theories "without bringing in meanings" are presented in a sketchy way and are, therefore, far from clear. So he writes (1965b, p. 232):

> We simply compare two infinite sets of elements with respect to certain structural properties and inquire whether or not an isomorphism can be established. Considering a Euclidean plane and an arbitrarily bent surface with

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[1] This may explain why it is possible for some authors to regard classical and relativistic mechanics as empirically incompatible and for others to regard them as incommensurable.

[2] According to K. Popper (1959, Ch. 6) two theories are not comparable with respect to their empirical content, if the classes of their potential falsifiers have no elements in common; but they do not have any elements in common, if the meanings of all extra-logical terms in basic statements vary from theory to theory. The same applies to other methods of comparing the empirical content of theories, e.g. to comparisons in terms of consequence relation. I. Lakatos (1968) seems to be using Popper's concept of empirical content in discussing changes in scientific theories; it is not clear whether he assumes invariance of basic statements throughout those changes.

Riemannian metric imposed upon it, we realize that there is nothing corresponding to a large-scale Pythagorean triangle on the latter which establishes that, in two dimensions at least, Newtonian mechanics and general relativity are different theories. . . .

The quoted passage seems to suggest that we may compare extensions or denotations of terms of two theories instead of their meanings. If this is the correct reading of part of the passage, then this idea can be expressed in a simpler way and it becomes obvious that it is not new at all. Because of the obscure nature of 'meaning' many prefer to replace it with 'extension' (e.g. Quine (1953), Scheffler (1967)) and this is the standard procedure in the model-theoretical approach (Przełęcki, 1969). However, not everything is acceptable in the quoted passage. Taken literally it suggests that whenever models of two theories or of some postulates of those theories are not isomorphic, the two theories are "different" or "incompatible". But isomorphism of models of two theories is not necessary to make them logically commensurable, nor is lack of isomorphism sufficient to make them incommensurable or incompatible. Any scientific consistent theory has models of arbitrary cardinality and so non-isomorphic. Any two theories whose models have at least some elements in common are logically comparable, since they may either agree or disagree in their claims about the common part of their models. What Feyerabend might have had in mind in the quoted passage was perhaps not lack of isomorphism but non-existence of common elements in the universe of models. However, this seems incompatible with his idea of "strong alternatives" as "partly overlapping" theories.

Feyerabend's second method of comparing theories concerns the empirical content of theories: 'Both theories may be able to reproduce the 'local grammar' of sentences which are directly connected with observational procedures' (1965b, p. 233) In such a case two theories, T and T', are comparable with respect to their empirical content through "local statements", which—though not identical in meaning—are "pragmatically equivalent", i.e. are asserted under the same circumstances whether one accepts T or T' (cf. also Smart (1968), pp. 81–2). Now, there does not seem to me to be any difference between saying that two statements are pragmatically equivalent in the above sense and saying, e.g. that their acceptance is governed by the same empirical meaning rules (Ajdukiewicz, 1934a) or that they have the same stimulus-meaning (Quine, 1960) or, finally, that one is an extension-preserving translation of the other. If so, the difference between Feyerabend's position and the position he rejects—i.e. the view of comparability of alternative theories through observational statements—is more terminological than real, the more so that from the viewpoint of the "dual-language" reconstruction of empirical theories so called purely observational terms and statements may just as well be outside the proper, theory-laden language of science (Przełęcki (1969), Ch. 11). On the other hand, unlike his opponents, Feyerabend does not explain at all how "local statements" are correlated with theoretical statements within T and T′, nor whether a theory whose language would not be able to reproduce the "local grammar" of sentences directly connected with observational procedures, would count as empirical.

Finally Feyerabend mentions the third method: 'We may construct a model of T within T' and consider its fate. The proof of the irrationality of $\sqrt{2}$ may be seen in this way' (1965b, p. 233). One might want at this point to mention alternative geometries as examples of "prima facie conflicting theories" the terms of which may be so interrelated that they become "systems for codifying the same things in different ways" (Nagel (1961), p. 252). However, Feyerabend insists that "within physics the construction in T' of a model $\bar{T}$ of T usually is not possible without violation of highly confirmed laws". So the third method seems not to be available after all.

To conclude, all three methods of comparing theories, which Feyerabend recommends, presuppose that extensions of terms are somehow fixed, otherwise there would be no models and nothing to compare. How exactly they are fixed is not explained by him. Since the first and third of his methods do not rely on ostensive procedures it seems fair to assume that the extensions are fixed by the "whole theoretical context". But, as was argued before, this would make theories tautologous unless the analytic and the empirical components are somehow distinguishable in them. So none of the three methods of comparison mentioned by Feyerabend solves or avoids this problem.

I shall now resume my analysis of the paradox of meaning variance, disregarding the claim that the conventional and the empirical elements of a theory are in principle indistinguishable.

As stated before, explication (7.1) is acceptable, provided clauses (*b*) and (*d*) in it are suitably modified. However, (7.1) alone does not imply any of the paradoxical statements (2)–(4). Essential for the paradox is the second conjunct in (7), i.e. the claim that the meanings of all primitive terms change with the change in any of the factors (*a*) through (*e*). But this second conjunct does not follow from the first at all, as the following considerations show. One of the factors which according to (7.1) determine meanings of primitives, is the set of axioms. Since there are equivalent and observationally equivalent axiomatisations of a theory, the question arises which changes in the axiom set result in meaning variance.

One indication is contained in statement (2) of the paradox, viz. the phrase 'two prima facie conflicting theories'. Let two theories, T_1 and T_2, be axiomatised by $A_1, \ldots, A_i, \ldots A_n$ and $A_1, \ldots \sim A_i, \ldots, A_n$ respectively. We may think of T_1 as some axiomatisation of the Euclidean geometry, in which A_i is the axiom of parallels. Theories like T_1 and T_2, as uninterpreted calculi, are *syntactically mutually inconsistent*. If syntactical inconsistency proved to be a sufficient condition for the meaning variance of all primitive terms, then we should have a precise explication of the phrase 'prima facie conflicting theories' and of statement (2), which would then be reformulated as follows:

(8) Given two theories, T_1 and T_2, which are mutually syntactically inconsistent, no term P in T_1 has the same meaning as P in T_2.

Syntactical inconsistency is what Fine (1967) had apparently in mind, when he claimed in his contribution to the discussion of the paradox that the theory of real numbers and the theory of rational numbers are inconsistent and yet one encompasses the other. For presumably the formula:

$$(9) \qquad (x)(\exists y)\,(y^2 = x)$$

is derivable in the former whereas its negation in the latter.[1]

But syntactical inconsistency alone is no good as a criterion of meaning variance of primitive terms, as our previous considerations have shown. For suppose axiom A_i in T_1 is not an M-postulate but an empirical hypothesis. Then no meaning variance occurs in the transition from T_1 to T_2 (we can now think of T_1 and T_2 as being two systems of physical or applied geometry). If, on the other hand, A_i contains both conventional or analytic and empirical components, then we have to re-axiomatise T_1 and T_2 in such a way that the two types of postulates are clearly distinguished and then see whether T_2 results from T_1 by negating an M-postulate or an E-postulate or both. Moreover we have to bear in mind that if T_2 results from T_1 by negating some meaning-postulate A_{Mi} in T_1, then there may be other primitive terms in T_1 unrelated by any meaning-postulates to those in A_{Mi} and their meanings will remain unaffected by this transition from A_{Mi} to $\sim A_{Mi}$ and so from T_1 to T_2. This suggests that we should reformulate (8) perhaps as follows:

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[1] Fine's own formulation, however, is different and, I think, unsatisfactory (p. 239): 'Consider the theory of real numbers and the theory of rational numbers. Whereas every positive real number has a square root, not so every positive rational. So the theories are inconsistent . . .' (p. 239). But there is absolutely no inconsistency between the statement 'every positive real number has a square root which is a real number' and the statement 'not every positive rational number has a square root which is a rational number' since the two statements refer to two non-identical universes of objects, the set of real numbers and the set of rational numbers. Fine seems to want to avoid logical terminology. However, his claim that there are theories which are formally mutually inconsistent and nevertheless such that one of them encompasses the other and in particular that the theories of real and of rational numbers are such theories, simply cannot be expressed adequately in the ordinary language in which uninterpreted formulae like (9) do not occur and the concept of syntactical inconsistency is not available.

(10) Given two theories, T_1 and T_2, with M-postulates $A_{M1}, \ldots A_{Mi}, \ldots, A_{Mm}$ and $A_{M1}, \ldots, \sim A_{Mi}, \ldots, \ldots, A_{Mm}$ respectively, the meanings of extralogical terms which occur in A_{Mi} and in $\sim A_{Mi}$ and of all terms which are related by M-postulates to the former, will not be identical in T_1 and T_2.

It is clear already that *if the second conjunct in* (7) *is replaced by* (10), *then none of the statements* (2) *through* (4), *essential for the paradox of meaning variance, follows and the paradox, therefore disappears.*

In order to derive (2) from (7.1) and (10) and so (3) and (4) from (7.1) and (10), one would have to assume, e.g. the following additional premisses:

(11) If T_1 and T_2 are "prima facie conflicting theories", i.e. are mutually syntactically inconsistent, then their meaning-postulates differ in some way.[1]

(12) Every primitive term in a theory is related by every meaning postulate to every other primitive term in the theory.

But (11) is false and so is (12). (11) is false because, as we have seen, T_1 and T_2 may be mutually syntactically inconsistent and yet T_2 may result from T_1 by negating not a meaning postulate but an empirical hypothesis in T_1. To see that (12) is false when applied to actual scientific theories, we would have to examine them in axiomatic form in which the M-postulates and the E-postulates are distinguished. It is not difficult, of course, to construct examples of theories for which (12) is false. It is perhaps sufficient to stress that there seem to be no good reasons why every theory should conform to (12). Besides (12) becomes even less plausible if the purely syntactical phrase 'terms related by M-postulates', which also occurs in (10), is replaced by a more satisfactory semantical criterion of 'meaning dependence', which takes into account the fact that the restrictions imposed on the admissible interpretations of the primitive terms by the axioms of a theory may be weaker or stronger.[2] If the restrictions imposed by the axioms on the admissible interpretations of two terms, P_1 and P_2, are very weak, then we have considerable freedom in altering the interpretation of one of them without thereby affecting the interpretation of the other. The semantical criterion of meaning variance might be formulated perhaps along the following lines:

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[1] It might be interesting to consider the following case: Let T and T' be formally mutually inconsistent; let 'P_{MT}' stand for the meaning postulates of T. An extension of T' formed by adjoining P_{MT} to it may be consistent and it may yield as a consequence that either some or all primitive terms of T which occur in P_{MT} are empty, i.e. have no (physical) referents. T and T' would be commensurable although all their primitive terms have different meanings and extensions.

[2] The idea of meaning-related and meaning-independent terms as well as the idea of a connected language and of an isolated part of a language were defined by K. Ajdukiewicz (1934a) in terms of axiomatic, deductive and empirical meaning rules which govern the acceptance of statements. Their semantical counterparts were defined in model-theoretical terminology by R. Wójcicki (1966).

(13) The class of different denotations assigned to a term P of a theory T in the models of the meaning-postulates T_M of T, i.e. in the domains in which all the meaning postulates of T are true, may be called the class of T_M-compatible interpretations of P. In the transition from T to another theory T', in which P also occurs, if the class of T_M-compatible interpretations of P is not identical with the class of T_M-compatible interpretations of P, then the meaning of P has changed, provided that direct, i.e. ostensive, interpretations of terms have not been altered.

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The Notion of Validity in Logical Systems with Inexact Predicates

by J. P. CLEAVE

1 KÖRNER (1966) has proposed a definition of logical validity of sentences with inexact predicates. In this paper we prove the formal viability of Körner's proposal by showing that it yields a notion of *deductive system* satisfying the postulates of Tarski (1956), Chapter V.

Körner's method can be illustrated by a classical propositional calculus L. Suppose L has propositional variables $p_0, p_1, p_2, \ldots$ Now consider a sentence $F(\bar{p}_0 \ldots \bar{p}_n)$ formed by substituting *fixed* propositions $\bar{p}_0 \ldots \bar{p}_n$ for the corresponding variables $p_0 \ldots p_n$ in a formula $F(p_0 \ldots p_n)$ of L. If the propositions $\bar{p}_0 \ldots \bar{p}_n$ are *definite*, that is either true or false, then it is natural to say that the constant sentence $F(\bar{p}_0 \ldots \bar{p}_n)$ is L-valid if and only if $F(p_0 \ldots p_n)$ is a valid formula of L (see Hilbert, Ackermann (1950)). Körner (1966) advances reasons for believing that this definition is inadequate when at least one of the propositions $\bar{p}_0 \ldots \bar{p}_n$ in indefinite, and proposes the following definition of validity. Let $\bar{p}_{i1} \ldots \bar{p}_{ik}$ be the definite propositions amongst $\bar{p}_0 \ldots \bar{p}_n$, and $\bar{p}_{j1} \ldots \bar{p}_{jm}$ the indefinite propositions. Let '$G(\bar{p}_{j1} \ldots \bar{p}_{jm})$' denote the results of deleting the indefinite propositions and the connectives governing them. Then $F(\bar{p}_0 \ldots \bar{p}_n)$ is valid in the new sense—L^*-valid—if and only if both $F(\bar{p}_0 \ldots \bar{p}_n)$ and $G(\bar{p}_{j1} \ldots \bar{p}_{jm})$ are L-valid in the usual sense.

A precise definition of a procedure for excision of indefinite propositions can be based upon the *Streichungsverfahren* of Schütte (1960), p.17. As examples of this procedure we have (i) the deletion of the subformula A from $((p_1 \wedge \neg A) \vee p_2)$ yields $p_1 \vee p_2$, and (ii) deletion of A from $\neg\neg A$ yields the empty sequence of signs, thus annihilating $\neg\neg A$. We refrain from quoting Schütte's definition, as the only property of the *Streichungsverfahren* used in the following theory is that, applied to any formula, the procedure yields a finite set (possibly empty) of other formulas.

The role of indefinite propositions can be restricted by the following device. Partition the variables of L into two distinct classes D, I—e.g. $p_0, p_2, p_4, \ldots \in D$ and $p_1, p_3, p_5 \ldots \in I$. In applying the pure logic L

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to fixed propositions with possibly indefinite atoms, we shall say that a constant proposition $\overline{F}$ is a ***special substitution*** instance of a formula F of L if $\overline{F}$ arises from F by substituting definite constant propositions for the D-variables of F and indefinite constant propositions for the I-variables of F. Further, let '$\partial(F)$' denote the result of excising the I-variables from F by the *Streichungsverfahren*. Then a constant proposition $\overline{F}$ is L^*-valid if and only if it is a special substitution instance of a formula F such that $\vdash_L F$ and $\vdash_L \partial(F)$. It is quite natural now to say that a formula F of L is L^*-valid if and only if $\vdash_L F$ and $\vdash_L \partial(F)$ so that a constant proposition is L^*-valid if and only if it is a special substitution instance of an L^*-valid formula. Thus, the criterion for L^*-validity of an L-formula concerns only the logic L and the syntactical excision operator ∂. For example

(i) $\vdash_{L^*} p_0 \vee \neg p_0$ because there are no I-variables in the formula so that $\partial(p_0 \vee \neg p_0) = p_0 \vee \neg p_0$. Obviously, $\vdash_L p_0 \vee \neg p_0$ and $\vdash_L \partial(p_0 \vee \neg p_0)$.

(ii) not $\vdash_{L^*} p_1 \vee \neg p_1$, because the only variable in this formula is the I-variable p_1: ∂ annihilates this formula and so not $\vdash_L \partial(p_1 \vee \neg p_1)$.

(iii) $\vdash_{L^*} (p_0 \vee \neg p_0) \wedge (p_1 \vee \neg p_1)$. For $\partial((p_0 \vee \neg p_0) \vee (p_1 \vee \neg p_1)) = (p_0 \vee \neg p_0)$. Hence $\vdash_L (p_0 \vee \neg p_0) \wedge (p_1 \vee \neg p_1)$ and $\vdash_L \partial((p_0 \vee \neg p_0) \wedge (p_1 \vee \neg p_1)$.

It should be observed here that if the formulas of L are evaluated by the strong 3-valued truth tables of Kleene (1962) then some formulas of L are L^*-valid although every special substitution instance has value u (undefined). For $p_0 \vee \neg p_0$ has value t for every special substitution since $p_0 \in D$ and so p_0 take either t or f as values. But $p_1 \vee \neg p_1$ has value u for every special substitution since $p_1 \in I$ and so p_1 always has value u. Hence $(p_0 \vee \neg p_0) \,\&\, (p_1 \vee \neg p_1)$, which is L^*-valid ((iii) above), always has value u.

Finally we introduce L^*-consequence by the definition: if F_1, F_2 are L-formulas, $F_1 \vdash_{L^*} F_2 \Leftrightarrow F_1 \vdash_L F_2 \,\&\, \partial(F_1) \vdash_L \partial(F_2)$.

Let S denote the set of formulas of L and S^* those formulas of L which are not annihilated by ∂. Then S together with the consequence relation $\vdash$ —i.e. the structure $\mathbf{D} = \langle S, \vdash_L \rangle$—is a *deductive system* in Tarski's sense (1956). One can readily check that in this case $\partial\mathbf{D} = \langle S^*, \vdash_{L^*} \rangle$ is also a deductive system. Our aim in the next section is to prove that this is a quite general phenomenon—namely, given a deductive system $\mathbf{D}$ and an excision operator ∂, then $\partial\mathbf{D}$ is also a deductive system. Furthermore, putting reasonable restrictions on ∂ ensures that certain logical features of $\mathbf{D}$ are inherited by $\partial\mathbf{D}$.

2 USING THE NOTATION '$\mathbf{P}(X)$' for the collection of subsets of a set X, and '$\mathbf{P}_\omega(X)$' for the collection of finite subsets of X, Tarski's definition of a deductive system can be expressed as follows. Let S be a set, C a mapping of $\mathbf{P}(S)$ into $\mathbf{P}(S)$. Then the structure $\langle S, C\rangle$ is a *deductive system* if

(D1) $\overline{\overline{S}} \leqslant \aleph_0$
(D2) $X \subseteq S \Rightarrow X \subseteq C(X) \subseteq S$
(D3) $X \subseteq S \Rightarrow C(C(X)) = C(X)$
(D4) $X \subseteq S \Rightarrow C(X) = \cup\{C(Y);\ Y \in \mathbf{P}_\omega(X)\}$.

If, in addition $\supset$ is a two-place function on S then $\langle S, C, \supset\rangle$ is a *deductive system with implication* if $\langle S, C\rangle$ is a deductive system, and

(D5) $x,y \in S, X \subseteq S \Rightarrow .\ x \supset y \in C(X) \Leftrightarrow y \in C(X \cup \{x\})$.

To understand how to define an excision operator on a deductive system we refer to the *Streichungsverfahren* and the logic L discussed in §1. It is reasonable to define ∂x to be the set of formulas y which result from application of the *Streichungsverfahren* to x. Thus in this case ∂x contains at most one member and the *Streichungsverfahren* annihilates x if, and only if, $\partial x = \phi$ (the empty set). Furthermore, if y results from x by applying the *Streichungsverfahren* to x, then applying it again to y yields y. Thus if $y \in \partial x$ then $y \in \partial y$. We therefore define an *excision-operator* ∂ on a deductive system $\langle S, D\rangle$ to be a mapping $\partial\colon S \to \mathbf{P}_\omega(S)$ with the property

(E1) $x,y \in S, y \in \partial x \Rightarrow \partial y \neq \phi$.

On analogy with the construction of $\partial\mathbf{D}$ in §1, if $\mathbf{D} = \langle S, C\rangle$ is a deductive system and ∂ an excision operator on $\mathbf{D}$, then $\partial\mathbf{D}$ is defined to be the structure $\langle S', C'\rangle$ where

(i) $S' = \{x;\ x \in S\ \&\ \partial x \neq \phi\}$
(ii) for all $x \in S'$ and $X \subseteq S'$,
$x \in C'(X) \Leftrightarrow .\ x \in C(X)\ \&\ \partial x \subseteq C(\partial X)$.

(Note. We use the notation $\partial X = \cup\{\partial y; y \in X\}$, $\partial^{-1}X = \{y; \partial y \subseteq X\}$.)

The first result is

Theorem 1 If ∂ is an excision operator on a deductive system $\mathbf{D}$, then $\partial\mathbf{D}$ is also a deductive system.

Proof Let $\mathbf{D} = \langle S, C\rangle$ be a deductive system with excision operator ∂. Then it is trivial to prove that $\partial\mathbf{D}$ satisfies D1 and D2. To prove D3, D4 we use the fact that C is monotone (Th. 1, Ch. V, Tarski (1956)) i.e,

$$X \subseteq Y \subseteq S \Rightarrow C(X) \subseteq C(Y). \tag{1}$$

Further, as ∂ satisfies E1, we have, using (1)

$$X \subseteq S' \Rightarrow \partial C'(X) \subseteq C\partial(X), \tag{2}$$

$$X \subseteq S' \Rightarrow C'(X) \subseteq C(X) \tag{3}$$

ad D3. Let $X \subseteq S'$. Then

$$\begin{aligned} C'C'(X) &\subseteq CC'(X) \text{ by (1), (3)} \\ &\subseteq CC(X) \text{ by (1), (3)} \\ &\subseteq C(X) \text{ by D3.} \end{aligned}$$

Thus

$$x \in C'C'(X) \Rightarrow x \in C(X) \tag{4}$$

Next,

$$\begin{aligned} x \in C'C'(X) &\Rightarrow \partial x \subseteq C\partial(C'(X)) \\ &\Rightarrow \partial x \subseteq CC\partial(X) \text{ by (1), (2)} \\ &\Rightarrow \partial x \subseteq C\partial(X) \text{ by D3.} \end{aligned}$$

Thus

$$x \in C'C'(X) \Rightarrow \partial x \subseteq C\partial(X) \tag{5}$$

From (4), (5)

$$C'C'(X) \subseteq C'(X) \tag{6}$$

As ∂**D** satisfies D2,

$$C'(X) \subseteq C'C'(X) \tag{7}$$

From (6), (7) ∂**D** satisfies D3.

ad D4. Let $x \in S'$, $X \subseteq S$ and $x \in C'(X)$. Then $x \in C(X)$ so by D4 there is set Y such that

$$x \in C(Y),\ Y \in \mathbf{P}_\omega(X) \tag{8}$$

Further $\partial x \subseteq C\partial(X)$. But ∂x is finite, so by D4 and (1), there is a $Z \in \mathbf{P}_\omega(\partial X)$ such that $\partial x \subseteq C(Z)$. But Z is finite. Hence there exists a $Y' \in \mathbf{P}_\omega(X)$ such that $\partial Y' \supseteq Z$. Hence by (1),

$$\partial x \subseteq C\partial(Y'),\ Y' \in \mathbf{P}_\omega(X) \tag{9}$$

Let $Y'' = Y' \cup Y$. Then $\partial Y'' \supseteq \partial Y'$ and $Y'' \in \mathbf{P}_\omega(X)$

Hence by (1), (8), (9)

$$x \in C(Y''),\ \partial x \subseteq C\partial(Y''),\ Y'' \in \mathbf{P}_\omega(X) \tag{10}$$

Thus, by (10)

$$C'(X) \subseteq \cup\{C'(Y);\ Y \in \mathbf{P}_\omega(X)\} \tag{11}$$

Now from (1) it follows that C' is also monotone. Hence $C'(X) \supseteq C'(Y)$ for all $Y \in \mathbf{P}_\omega(X)$. Hence from (11)

$$C'(X) = \cup\{C'(Y);\ Y \in \mathbf{P}_\omega(X)\}.$$

Thus ∂**D** satisfies D4.

A condition on ∂ which is sufficient to ensure that ∂**D** satisfies D5 is:

> (E2) There exists a function $z(a,x,y)$ on S such that if ∂x, $\partial y \neq \phi$ then $\partial(x \supset y) = \{z(a,x,y);\ a \in \partial y\}$ and for each $a \in \partial y$ there exists a sequence $(x_1, \ldots x_n)$ such that $(x_1 \ldots x_n) = \partial x$ and $z\ (a,x,y) = (x_1 \supset \ldots \supset (x_n \supset a) \ldots)$.

It will be noted immediately that if $\mathbf{D} = \langle S, C, \supset \rangle$ is a deductive system with implication, and ∂ is an excision operator on **D** satisfying (E2) then

$$\partial x,\ \partial x \neq \phi \Rightarrow \partial(x \supset y) \neq \phi.$$

Hence S' is closed under $\supset$.

Theorem 2 Let **D** be a deductive system with implication and ∂ an excision operator on **D** satisfying (E2). Then ∂**D** is also a deductive system with implication.

Proof Let **D**, ∂ satisfy the hypothesis of the above statement. Let $x, y \in S'$ and $X \subseteq S'$. We have to prove

$$x \supset y \in C'(X) \Leftrightarrow y \in C'(X \cup \{x\}) \qquad (12)$$

(i) Suppose $x \supset y \in C'(X)$. Then $x \supset y \in C(X)$ so as **D** satisfies D5

$$y \in C(X) \cup \{x\}) \qquad (13)$$

Also $\partial(x \supset y) \neq \phi$ and $\partial(x \supset y) \subseteq C\partial(X)$. We show that

$$\partial y \subseteq C\partial(X \cup \{x\}) \qquad (14)$$

Suppose $a \in \partial y$. By (E2) there exists a sequence $(x_1 \ldots x_n)$ such that $\{x_1 \ldots x_n\} = \partial x$ and $z(a, x, y) = (x_1 \supset \ldots \supset (x_n \supset a) \ldots) \in \partial(x \supset y)$. Thus $(x_1 \supset \ldots \supset (x_n \supset a) \ldots) \in C\partial(X)$. Hence, as **D** satisfies D5, $a \in C(\partial(X) \cup \{x_1 \ldots x_n\}) = C(\partial(X) \cup \partial x) = C\partial(X \cup \{x\})$. This proves (14)

By (13), (14) $x \in C'(X \cup \{x\})$. Thus

$$x \supset y \in C'(X) \Rightarrow y \in C'(X \cup \{x\}) \qquad (15)$$

(ii) Conversely, suppose $y \in C'(X \cup \{x\})$. Then $y \in C(X \cup \{x\})$. As **D** satisfies D5

$$x \supset y \in C(X) \qquad (16)$$

Also, $\partial y \subseteq C\partial(X \cup \{x\}) = C(\partial X \cup \partial x)$. Suppose $q \in \partial(x \supset y)$. By (E2), $q = (x_1 \supset \ldots \supset (x_n \supset a) \ldots)$ for some $a \in \partial y$ and sequence $(x_1 \ldots x_n)$ such that $\{x_1 \ldots x_n\} = \partial x$. But then $a \in C(\partial X \cup \{x_1 \ldots x_n\})$. As **D** satisfies D5, $q \in C\partial(X)$. Thus

$$\partial(x \supset y) \subseteq C\partial(X) \qquad (17)$$

By (16), (17), $x \supset y \in C'(X)$. Thus,

$$y \in C'(X \cup \{x\}) \Rightarrow x \supset y \in C'(X) \qquad (18)$$

(12) now follows from (15) and (18)

3 (A) A SIMPLER FORMULATION of condition E2 is possible where ∂ is an excision operator arising from a process of deleting subformulas. For in this case the *Streichungsverfahren* either annihilates a formula x or produces a single formula from it. Thus for all x, $\overline{\overline{\partial x}} \leqslant 1$. Hence we define a partial function ∂^* on the formulas in S by

$$\partial^* x = (\iota y)(y \in \partial x)$$

Then (E2) is equivalent to:

$$(\text{E}2')\text{: } \partial^* x,\ \partial^* y \text{ defined} \Rightarrow \partial^*(x \supset y) = \partial^* x \supset \partial^* y.$$

(B) Our procedure is quite general—if one can formulate a notion of "indefinite proposition" for an arbitrary logic L in such a way as to justify an excision operator ∂ on L, then provided ∂ has the properties

E1 and E2 one can construct from L a notion of logical consequence. We add that the logic L is not restricted to propositional calculi. For given a pure predicate logic L, the predicate variables can be partitioned into classes D and I and *special substitution instance* defined accordingly, that is, the variables D reserved for substituting exact (i.e. definite predicates) and I for substituting inexact predicates.

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Discussions

REPLY TO PROFESSOR GRÜNBAUM

Professor Adolph Grünbaum (1969) has been so flattering as to reply at great length to my short paper (1968). I do not find his rejoinder convincing in any particular. But I shall not attempt to rebut all his various points. This would involve controversial expansion, and an even longer reply than his. Thus, with every prospect of further rejoinder, we should get into an infinitely divergent series of thesis and antithesis; which could satisfy no one, least of all the Editor.

I shall therefore confine myself to the single point: that Grünbaum has failed to substantiate his basic contention which I attacked, and which he expressed in the following way (1968, p. 17):

> It seems to me of *decisive* significance that no cognisance is taken of nowness (in the sense associated with becoming) in any of the extant theories of physics. If nowness were a fundamental property of physical events themselves, then it would be very strange indeed that it could go unrecognised in all extant physical theories without detriment to their explanatory success.

In his reply Grünbaum refers (p. 148) to my "attempt to base an objective physical nowness on the 'inherent one-sidedness' which characterises Norbert Wiener's *temporally* asymmetric examples . . ." I did not in fact attempt to base any part of my argument *solely* on any characteristic of "inherent one-sidedness". Asymmetry of the temporal ordering relation is indeed in my view a *necessary* condition for the existence of an objective characteristic of physical presentness, "nowness", or becoming; but I have never said that it was a *sufficient* condition. However I shall restate my argument in a slightly different form, which still turns on the physics of communication theory, but is *not* specifically tied to Wiener's theory of prediction. The actual technical procedures used in certain branches of physics—specifically communication theory and its applications—show unequivocally that physicists have found it necessary to draw the same kind of distinction as McTaggart drew; between his A-series and B-series of terms related by temporal characteristics. In Chapter 33 of his (1927) McTaggart draws a distinction between two kinds of time-series:

(*a*) the *A*-series, which is simply the series of events ordered in respect of the *A*-characteristics of strict presentness (i.e. nowness), and of various degrees of pastness and futurity.

(*b*) The *B*-series which is simply the series of events ordered by the relation 'earlier than' or 'later than'.

McTaggart (1927) maintained that unless events which are the terms of a series have *A*-characteristics, and can change in respect of these objectively and independently of anyone's perceiving them, the series cannot be a process of change; and he says: 'There could be no time if nothing changed.'

Now Grünbaum would apparently accept McTaggart's *B*-series as having objective reality independently of any mind-dependent experience. But he would reduce the *A*-characteristics to those generating the *B*-series. For example he says:

> To be future at time t_0 just means to be later than t_0 which is a tenseless relation, and obtains at *all* times if it obtains at all at some one time. By the same token, to be past with respect to t_0 is merely to sustain the tenseless relation of being earlier to the time t_0.

This is the essence of the difference between those who hold that becoming is mind-dependent, and those, like myself, who hold that there is more to past and future than simply the relation of 'earlier than' or 'later than' a given time point t_0. We hold that there is an essential difference between what some physicists have called 'static time', as opposed to what they call 'running time'.

In any case it seems that a stronger sense than Grünbaum's has to be given to the *A*-characteristics in certain current physical theories, as indicated in the following quotations taken from two leading practitioners of Communication Theory, Professors Colin Cherry and Denis Gabor, of Imperial College, London. Both these quotations are concerned with essentially the same point: the need to use different mathematical models to explain the difference between the situations in which the physical theory takes account of changes in the *A*-characteristics and those in which only the *B*-series features are significant. The latter obtain when *all* the values of a function of time are assumed to be knowable, throughout the infinite range of time from $t = -\infty$ to $t = +\infty$; and the question of which particular instant, t_0 say, is *present*, is irrelevant to the physical problem under consideration. Such is the situation where the classical principles of frequency analysis, in terms of Fourier's theorem, are used to derive the theoretical frequency spectrum of some signal $S(t)$ given as some function of *all* time, e.g. in terms of the variation of voltage with time taken from

$$t = -\infty \text{ to } t = +\infty.$$

But this theoretical situation is clearly distinguished in current physics from another, in which we know the behaviour of $S(t)$ only over a restricted range of the variable t, viz. only over a *certain period before the present instant or 'now'* As Cherry remarks (1962, p. 139):

> [Though] The classical principles of Fourier analysis have served the communication engineer ... for many years ... signals must be known for all time "static time", before they can be analysed into spectra in the way indicated. But it is characteristic of communication signals that we can only have access to their *past* values, as functions of "running" time; their exact forms, in the future, are not known with certainty—otherwise, it can be argued, there would be no need to communicate them. Considerable ingenuity has been shown, for example by Gabor and Fano, in extending the concepts of Fourier analysis so as to take account of this difficulty.

I shall now turn to a relevant passage in the work of Gabor where he in turn, refers to the work of Fano. In the following quotation Gabor is dealing with what he calls 'live' signals, where the time of reception is significant (as it determines the time of the decoding process), as distinct from 'dead' signals, where the time of reception has no significance with reference to the decoding process. He remarks (1953, p. 9):

> ... the present instant is, on the whole, the most important while the importance of the past fades out continuously.... This suggests a 'perspectivic' weighting of the past, which has been considered by Fano.

The simplest example is exponential weighting of the past. As t is now reserved *for the present instant*, let $\tau<t$ be used for *past* instants.

Replace $S(t)$ by

$$s(\tau)e^{-\alpha(t-\tau)} \qquad (a>0,\tau<t)$$

When correspondingly the amplitude samples are now

$$S_n = S\left(t-\frac{n-\frac{1}{2}}{2W}\right)e^{-\alpha[t-(n-\frac{1}{2})/2W]}\ .$$

> ... the symmetry with the Fourier representation is destroyed because the spectrum is now a continuous function of the frequency f. It is by Fourier's cosine integral theorem
>
> $$S(f,t) = 2\int_{-\infty}^{t} e^{-\alpha(t-\tau)}s(\tau)\cos 2\pi f(t-\tau)d\tau.$$

(My italics throughout.)

It is the need to introduce some form of "weighting" function, in terms of time elapsed from "the now" or "present instant" (along the lines indicated in the passage quoted) in order to modify the "dead" signal function (so as to give adequate expression to its difference from a "live" signal), which shows how the physicist does indeed require the concept of "nowness in the sense of becoming". He needs the concept just because a basic mathematical difference between the classical Fourier analysis, representing signals in "static time" and the modifications introduced by Gabor and Fano, is required to express the fact that "live" signals "happen in the tensed sense of coming into being apart from anyone's awareness of them" (Grünbaum (1968), p. 22) An example of such "live" signals would be the successive presentation, frame by frame, of a film on an *unwatched* television screen, which had however been switched on.

In logic a universal statement only requires a single counter example to refute it. I would claim that the foregoing example is therefore sufficient to refute Grünbaum's basic contention, on which his theory of the mind-dependence of "becoming" seems to rest.

I imagine no one will be so naive as to suppose that, because my discussion has involved the phrases 'live' and 'dead' in connection with signals, the argument necessarily involves the presence of what Grünbaum calls a 'mind-possessing organism'. Such a presence is no more necessarily involved in the foregoing than in any other discussion in physics, where always, of course, the existence of "mind-possessing organisms" is in a sense always implicit in the background: in that such organisms are required if there is to be any possibility of observation and experiment to test the theory.

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Since receiving this note we have learned with regret of Mr. Dobbs' death.

GRÜNBAUM *vs.* DOBBS: THE NEED FOR PHYSICAL TRANSIENCY

1 THE GRÜNBAUM—DOBBS CONTROVERSY

In his "Reply to H. A. C. Dobbs" (*Brit. J. Phil. Sci.* **20** (1969), 145–53), Adolf Grünbaum spiritedly defends his theory of the mind-dependence of temporal becoming against several criticisms, the last of which deserves further discussion.

Dobbs' crucial objection is to the absence in Grünbaum's theory of any objective counterpart to the mental "now". For Grünbaum the physical universe is intrinsically devoid of past, present, or future, though ordered by temporal relations of earlier and later. Events do not in fact "come to be" but exist (occur) tenselessly. Their seeming transiency is wholly supplied by the minds of observers, who, by being propositionally aware of events and of their awareness, constitute the "now".

Against this Dobbs argues that such an account fails to provide any intelligible basis for the fact that

> . . . we all have an agreed and working practical simultaneity in our experience. . . . But unless this agreement is founded on some common objective counterpart to the mental 'now', which *causes* our individual brains to register 'now' with practical simultaneity, it is impossible to see why *I* should not be contrary, and hold that 'now it is twelve noon on Queen Anne's birthday in the year 1700'. In other words, unless the *mental becoming* of the experience of the specious present is occasioned by the *physical becoming* present of the neurophysical events underlying consciousness, the fact of intersubjective practical simultaneity would be explicable only in terms of a miraculous pre-established (spiritualist) harmony (Dobbs, 1969, 321–2).

In this criticism Dobbs has come close to exposing a fatal weakness in Grünbaum's position; but, unfortunately, his statement of it is flawed in such a way as to allow Grünbaum to evade its force.

Grünbaum's reply consists in three points:

(1) Effects need not resemble their causes. Mental events, although characterised by awareness of presentness, may therefore be held to be wholly the effects of physical causes without imputing to such causes any intrinsic property of presentness.

(2) The mental events of several percipients, if *ex hypothesi* caused practically simultaneously by intrinsically tenseless physical events, should be expected to show inter-subjective agreement on matters of tense.

(3) The possibility of Dobbs' judgment at any time in his lifetime that it is Queen Anne's birthday in 1700 is plainly false, since "there simply is ***no*** event in Mr Dobbs' lifetime that is ***simultaneous*** with any event in the year 1700" (Grünbaum, 1969, 153).

I shall deal with each of these in inverse order.

2 PROBLEMS IN SUBJECTIVE BECOMING

I agree, first, that in (3) Grünbaum has picked up a regrettable error in Dobbs' statement of the argument, but I shall argue that Dobbs' essential point, obscured by his unhappy choice of illustration, remains strong. Leaving Queen Anne aside, suppose, rather, that it is true to say of me that as constituting part of my life's career a "now"-awareness event of Grünbaum's specified kind, A_1, exists (occurs tenselessly) at clock time t_1, and that it is also true in my life's career that a different "now"-awareness event, A_2, exists (occurs tenselessly) at clock time t_2. In the universe depicted by Grünbaum both events have equal claim to existence; neither clock time t_1 nor t_2 has any intrinsic claim to privileged status as more "really" now (whatever, to Grünbaum, that phrase could mean) than the other. Suppose, further, that it is a phenomenological fact that A_2 fills my subjective field of awareness, not A_1. Why?

It will not do for Grünbaum to answer that I am uniquely experiencing A_2 simply because it is clock time t_2. Objectively speaking, on his view, it is (tenselessly) no less clock time t_1, and at time t_1 a "now"-awareness event A_1 no less genuinely exists (occurs) for me. Yet I find myself involuntarily discriminating against time t_1 and its associated event A_1 ***with no objective ground whatever*** to account for this.

To avoid the radical arbitrariness implicit in this result Grünbaum has two avenues of escape: (*a*) he can admit some feature of intrinsic transiency into his portrayal of the objective world, which would be to forfeit his entire position; or (*b*) he can postulate a disconnected realm of spiritual dynamics with mysterious but lawful powers of becoming, which would be to give up his firmly held tenet that mental events are wholly caused by physical ones. Neither alternative would be appealing to Grünbaum, but since the latter would at least leave current physical theory untouched, it is easy to see why Dobbs could assume that "extreme mentalism or spiritualism" might for Grünbaum have been the lesser evil.

3 PROBLEMS IN INTERSUBJECTIVE BECOMING

Thus Grünbaum's reply in (2), that the intersubjectivity of the present is causally determined no more problematically than is agreement on any other mind-dependent quality, will no longer suffice: He says:

> The mind-dependence of becoming is no more refuted by such intersubjectivity in regard to tense than the mind-dependence of common-sense colour attributes is in the least disproved by agreement among several percipients as to the colour of a chair (1969, 152).

In the case of several persons coming to agreement about the colour of a chair, however, the very possibility of their common reference—what makes the chair a "public object" for them—requires the shared temporal awareness that is here precisely at issue. But since an objective temporal framework of earlier-later relations is not sufficient to determine any unique experiential location for the minds involved, Grünbaum's analogy merely presupposes (without offering a basis for) the fact of intersubjective copresence of experience—unless he covertly assumes some privileged objective ground for the common experienced "now" (or some occult process of spiritualistic harmonising).

4 GRÜNBAUM'S BASIC INCOHERENCE

For Grünbaum it is theoretically forbidden to say of any person that he is more truly at any clock time than at any other in his lifetime. Since our minds are no less part of the natural order than our bodies, this must apply *a fortiori* to mental events as well. But the phenomenological fact is that for subjective awareness some mental events are uniquely "favoured" over others (in the sense of being judged "now"). Thus a systematically incoherent division is created between the order of *mental events conceived as occurring tenselessly*, on the one hand, in a perfect democracy of earlier-later relations wherein none has special claims or privileges, and the order of *mental events experienced as occurring transiently*, on the other hand, in a remorselessly ruled public succession of present moments.

This theoretical incoherence is fatal for any attempt at a unified understanding of the universe on Grünbaum's principles. His point (1), above, urging that causes and effects need, after all, bear no similarity, rings hollow in this context, as a rather empty appeal to ignorance.

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MORPHOSPECIES AND BIOSPECIES: A REPLY TO RUSE

MICHAEL RUSE (1969) in the very title of his paper 'Definitions of species in biology' blurs a distinction which is central to the issues under discussion. Is he concerned with defining the names of individual species or with defining the term 'species'? Ruse says that one of the problems with species is deciding on criteria "for the inclusion (or exclusion) of a particular organism into a paticular species". This is not the task of a definition of 'species' but of the definition of the name of the individual species. Biologists of all persuasions agree that the names of individual taxa (including species) are defined in terms of clusters of characters (usually morphological). Is the individual two-legged, relatively hairless, etc.? If so, then he is probably a member of the taxon *Homo sapiens*.

Definitions of the term 'species' are designed to aid in deciding which taxa are to be considered species. According to the biospecies definition, interbreeding relations are used to make these decisions. Is this taxon defined in terms of one cluster of characters reproductively isolated from other disjunct taxa defined in terms of other clusters of characters? If so, then according to the biospecies definition, it is a species. According to the morphospecies definition, a certain degree of morphological similarity and/or difference is used. Does this taxon defined in terms of one cluster of characters exhibit a certain degree of overall morphological similarity and/or difference from other disjunct taxa defined in terms of other clusters of characters? If so, then according to the morphospecies, it is a species. Ruse attempts to resolve the conflict between the advocates of the biospecies·definition and the morphospecies definition by observing that these two definitions provide alternative criteria for distinguishing roughly the same class of classes. In fact, the existence of two, logically independent criteria which, nevertheless, distinguish roughly the same groups is very good evidence that species are in some sense real. Biologists stress the biospecies definition because reproductive isolation is central to evolutionary theory. The morphospecies definition also distinguishes roughly these same units because there is a causal connection between interbreeding and character cohesion and dispersal.

As attractive as Ruse's resolution of the dispute between the advocates of these two definitions of 'species' might be, it will not stand up to careful analysis. Ruse assumes without argument that there is something called the *total* characteristics of an organism and that there is some *one* function which can integrate these characters to permit the delimitation of morphospecies which are roughly equivalent to biospecies. Although early in the current dispute between the proponents of these two definitions of 'species', the advocates of the morphospecies definition shared this assumption with Ruse, they have now come to see that both notions are ephemeral. (Ghiselin (1966), and Ehrlich (1967)). They have been forced to recognise more from empirical considerations than from philosophical arguments that any organism can be analysed into indefinitely many characters and that given any one set of characters indefinitely many coefficients of similarity can be used to partition them.

A weaker interpretation of Ruse's main thesis might be that among the many possible morphospecies definitions, there must be one which denotes roughly the same taxa as species as does the biospecies definition. Even though there are many borderline cases "evolutionary theory leads us to expect that if something is a biospecies, then it will also be a morphospecies based on many differences (and vice-versa)". Ruse could mean several things by this weaker assertion. If he means that for every biospecies thus far recognised, distinguishing features have always been discernible (no matter how slight), then he is correct, but this observation is relevant to defining the names of individual species, not to defining 'species' itself. Discovering slight morphological differences between sibling species helps biologists to distinguish one sibling species from another. It is of no help in the task of distinguishing species from subspecies, varieties, genera, families, and so on—the task of definitions of 'species'.

Ruse might mean, however, that in a majority of the cases, one integrative function will produce morphospecies which are roughly equivalent to biospecies. If so, then as far as the evidence to date is concerned, he is mistaken. Even excluding the troublesome borderline cases (sibling species and polytypic species) and limiting ourselves to "good" species, it is not true that one function will successfully delimit morphospecies which are roughly equivalent to biospecies. For any biospecies, there is always *some* function what will do it, but it varies from species to species. Perhaps this is all Ruse means by *the* morphospecies based on *the* total characteristics. If so, then he has worded his main thesis in an extremely misleading way. It is not true that there is a rough 1-1 correspondence between interbreeding groups which are reproductively isolated and groups with overall morphological similarity. There is a rough 1-many correspondence. Ruse might agree with my assertion about the illusive nature of *the* morphospecies but counter that there is no such thing as *the* biospecies either. If so, then so much the worse for his general thesis. We now have a many-many correspondence.

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ON IRRELEVANT CRITERIA OF CONFIRMATION

1 PROFESSOR MACKIE (1969) advances and discusses at length three seemingly very plausible criteria of confirmation.[1] The most basic of these is:

C: b confirms h in relation to k if and only if $p(b/k\&h)>p(b/k)$.

Two natural extensions of the above, he claims, are:

C_1: Of two possible observations b_1 and b_2, each of which would confirm h in relation to k, b_1 confirms h better than b_2 if and only if the adding of h to k raises the probability of b_1 more than it raises the probability of b_2, i.e.

$$\frac{p(b_1/k\&h)}{p(b_1/k)} > \frac{p(b_2/k\&h)}{p(b_2/k)}$$

C_2: If two alternative hypotheses h_1 and h_2, each of which would be confirmed by b in relation to k, h_1 is confirmed better than h_2 by b if and only if the adding of h_1 to k raises the probability of b more than the adding of h_2 to k does, i.e.

$$p(b/k\&h_1)>p(b/k\&h_2)$$

Let us see whether indeed these criteria hold in science:
Let 'k' stand for present day physics, the hypotheses be

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[1] In the latter part of his paper he adds the qualification 'direct' to confirmation to distinguish the following three criteria from other conditions which may be sufficient for other forms of evidential support. Throughout this paper 'confirmation' is brief for 'direct confirmation', unless otherwise specified.

h_1: The velocity of light $= \mathrm{c}$

h_2: The velocity of light $= \mathrm{c} + A \sin \dfrac{(K-V)!\Pi}{n}$

and 'b' stand for all the relevant observations.

Let V stand for the relative velocities of two specified galaxies measured in given units to the nearest integer and both K and n are integers. As long as $(K-V)>n$, $\dfrac{(K-V)!}{n}$ is an integer and $\sin \dfrac{(K-V)!\Pi}{n} =$ zero. Because of the expansion of the universe the relative velocities of galaxies increase all the time. Suppose that in 1975, for the first time in history, V reaches the crucial value for which $K-V<n$ and, therefore, according to h_2, the velocity of light will, from then onwards, be observed to vary.

All our observations made so far about the behaviour of light—which we shall denote by 'b'—are, of course, entailed just as much by h_2 as they are by h_1 and thus $p(b/h_2\&k) = 1$ and therefore $p(b/h_2\&k)>p(b/k)$. We can, nevertheless, be quite certain that Professor Mackie holds with everyone else that past observations confirm, to a very high degree, that the velocity of light is constant and that they do not provide any support for h_2. Does this not show that his basic criterion C, according to which whenever $p(b/k\&k)>p(b/k)$ then b confirms h in relation to k, fails to hold?

Not necessarily. Mackie could defend himself by saying that C holds universally and consequently b confirms h_2 as well as it does h_1. We have however, no inclination to *accept* h_2 in view of the fact that b confirms h_1 much more strongly than it does h_2.

This defence implies, however directly, the falsity of the claim that C_2 is applicable in science. In our example $p(b/h_1\&k) = p(b/h_2\&k)$, for both expressions equal unity and, according to C_2, b confirms h_2 to the same degree as it does h_1. Yet h_1 is regarded much more confirmed by b than h_2. Mackie's contention that unless $p(b/h_1\&k)>p(b/h_2\&k)$, b does not confirm h_1 more than h_2, is false. In practice scientists regard h_1 more confirmed by b since everything being equal, h_1 accounts for b in a much more economic fashion than does h_2.

The example quoted is more than just a counterexample to the contention that $p(b/h_1\&k)>p(b/h_2\&k)$ is a sufficient and necessary condition for h_1 to be confirmed better by b than h_2. The example represents the general case in science. In other words, it is not merely are hypotheses not always judged better confirmed on the basis of C_2 but no hypothesis in science is pronounced more confirmed than its rivals on the basis of C_2. C_2 has no use whatever in empirical reasoning.

To see the complete generality of the objection I have raised, let us be reminded that, given any finite set of observations, there is always an infinite number of hypotheses which entail it. For suppose we found that the hypothesis postulating that the law governing the co-variation of the physical parameters p and q is represented by $q = f(p)$ is confirmed by b, since b, which states that the results we obtained from measuring the values of q corresponding to a number of different values of p are all implied by the expression $q = f(p)$. It is then invariably true that these same results are also entailed by $q = f(p) + X$ where X is a function of p and any additional parameter and is of the form such, that $X = 0$ for all past observations but not necessarily for any further observations. It is trivial to show that X which satisfies these conditions may, in every case, assume infinitely many forms. For each one of the hypotheses H which postulate that the co-variation of p and q is governed by $q = f(p) + X$, it is true that $p(b/H\&k) = 1$. Thus if scientists indeed adhered to C_2, they would never subscribe to any hypothesis since there are no circumstances under which a particular hypothesis does not have many rivals which, on the basis of C_2, are to be regarded as confirmed equally by the evidence.

But what happens in practice is, that scientists choose out of the infinitely many hypotheses which imply b, the unique one in which $X = 0$ for all observations, actual and possible. This practice is a particular application of the principle of simplicity, an application carried out virtually automatically, universally. Thus rather than using the principle that $p(b/h_1\&k)$ must exceed $p(b/h_2\&b)$ before b can be regarded as confirming h_1 more strongly than h_2, a principle which would prohibit all scientific activity, we maintain that, everything being equal, the hypothesis which accounts most economically for the phenomena is to be regarded as confirmed by them to the highest degree.

It is interesting to note that an objection to C_2 may be raised not only in scientific reasoning, but even in reasoning where the number of possible hypotheses is restricted. Consider the following example in which:

k: From a pack of five cards numbered 1 to 5, three cards have been drawn
h_1: The cards drawn are numbered 1, 2 and 3
h_2: 1 and 2 are among the cards drawn
b: 1 and 2 are among the cards drawn.

Here we have $p(b/k\&h_1) = p(b/k\&h_2) = 1$ and, according to C_2, b confirms h_2 no more than it confirms, which h_1, is absurd: b entails h_2, while it only raises the probability of h_1.

Possibly Mackie might attempt to mitigate this difficulty in the following way: He would point out that $p(h_1/k) = 1/10$; $p(h_1/k\&b) = 1/3$; $p(h_2/k) = 3/10$ and $p(h_2/k\&h) = 1$ from which it follows that

$$\frac{p(h_1/k\&b)}{p(h_1/k)} = \frac{p(h_2/k\&b)}{p(h_2/k)} = \frac{10}{3}.$$

It is seen, therefore, that b raises the probability of h_2 by the same factor it raises the probability of h_2. If ultimately the probability of h_2 is 1, while the probability of h_1 is only $1/3$, this is because initially, without being given b_1, the probability of h_2 was three times higher than that of h_1. This may lend some plausibility to the claim that b confirms h_2 to the same degree as it does h_2. But in view of the fact that b renders h_2 certain while it raises the probability of h_1 to $1/3$, it would seem odd to insist on this claim.

2 THE FACT that there are always infinitely many hypotheses equally well accounting for a given set of data and that it is not on the basis of any probability relationship how we decide which of these we adopt, refutes not only the contention that C_2, but also that C_1, applies in science.

Suppose b_1 denotes a given set of observations implied by h_s and h_c, where h_s is the simpler and h_c the more complex hypothesis. It is always possible to give a description of b_2, another set of logically possible observations, which is entailed by h_c but which is incompatible with h_s and is incompatible with any hypothesis simpler than h_c. If the observations denoted by b_2 were actually made, they would be taken as providing maximum support to h_c (i.e., confirming h_c more than all the hypotheses which are even more complex than h_c and also entail b_2). On the other hand, h_1, although implied by h_c, will be considered as supporting h_s more than it does h_c. Here we have then

$$p(b_1/h_c\&k) = p(b_2/h_c\&k) = 1$$

and thus presumably

$$\frac{p(b_1/h_c\&k)}{p(b_1/k)} = \frac{p(b_2/h_c\&k)}{p(b_2/k)}$$

and yet h_c is regarded more confirmed by b_2 than by b_1, although C_1 requires us to treat b_1 and b_2 as equally confirmatory with respect to h_c.

The only way, it seems to me, in which Mackie could defend C_1 is to undertake the quite hopeless task of arguing that although $p(b_1/h_c\&k) = p(b_2/h_c\&k)$, nevertheless, it turns out *always* that $\frac{p(b_1/h_c\&k)}{p(b_1/k)} > \frac{p(b_2/h_c\&k)}{p(b_2/k)}$ since, in all cases of actual or possible observations, $p(b_1/k) > p(b_2/k)$. How could one go about arguing for such an unlikely position? It might seem that one could claim, or perhaps more correctly, postulate, that the more simple an hypothesis, the greater the prior probability that it is true. h_s, being simpler than h_c, is more probable than h_c; but h_s entails b_1 and is incompatible with b_2, which is entailed by h_c, hence on certain assumptions the prior probability of b_1 is greater than that of b_2.

Such an argument seems doomed to failure from the start. As we know, there are philosophers who hold that the prior probability of all universal hypotheses is zero and others, who claim that one cannot meaningfully attach any probabilities to universal hypotheses. Mackie may, of course, disagree; yet in the case of at least some type of hypotheses, it is too hard to see how he can refuse to acquiesce to these views and how he can maintain that finite prior probability attaches to them. Suppose h_s is

$$\text{velocity of light} = c + A \sin \frac{(K-V)!\Pi}{n}$$

while h_c is even a more complex hypothesis, according to which the velocity of light is a function, not only of V, but of additional physical parameters as well. I cannot see how one could defend the claim that h_s has finite prior probability. Not only are there infinitely many functions of V, which could replace $A \sin \frac{(K-V)\Pi}{n}$ and not only can K assume infinitely many values, but V can stand for the relative velocities of infinitely many galaxy pairs. Admittedly, we can observe only a finite number of galaxies but there might be infinitely many of them and any of these may potentially be observable. Then again 'V' might represent infinitely many other physical parameters. Thus h_s is one of infinitely many hypotheses of *comparable complexity*. It is perhaps possible to maintain that the prior probability that the way light travels is correctly described by an equation of the form to which h_s belongs, is finite. But then whatever this finite amount is, it has to be divided equally among all the infinitely many hypotheses of the form of h_s. The prior probability of h_s can have, therefore, no finite value. Thus $p(h_s/k)>p(h_c/k)$ and consequently we have no argument why always $p(b_1/k)>p(b_2/k)$. Yet it is always the case that, given two sets of observations, b_1 which is entailed by a complex hypothesis as well as by a more simple one, and b_2 which is incompatible with the simpler hypothesis and is entailed by the complex one, then we consider b_2 as more effectively confirming the complex hypothesis than b_1. We conclude, therefore, that Mackie's contention that one set of observation (b_2) does not confirm an hypothesis (h_c) more than another set of observations (b_1), unless

$$\frac{p(b_2/h_c\&k)}{p(b_2/k)}>\frac{p(b_1/h_c\&k)}{p(b_1/k)}$$

i.e. unless

$$\frac{1}{p(b_2/k)}>\frac{1}{p(b_1/k)} \text{ (in case } h_c \text{ entails both } b_1 \text{ and } b_2\text{)}$$

i.e. unless $p(b_1/k)>p(b_2/k)$, is a mistaken one.

3 IT MAY SEEM conceivable perhaps that Mackie should wish to counter the major objection to C_2 and the objection against C_1 raised in the previous sections by insisting that observations which are entailed by a simple and by a complex hypothesis do not confirm the former more than they confirm the latter. Admittedly, everything else being equal, we end up assigning greater credibility to the simpler hypothesis, not, however, because it is better confirmed by the evidence, but because of the principle of simplicity. In other words, the principle of simplicity must not be looked upon as a principle determining which hypothesis is better confirmed by the evidence; it is a separate principle operating on its own, determining which of equally confirmed hypotheses should be assigned higher credibility.

This will, however, not do. Suppose we held that confirmation and acceptability are two separate notions. Suppose also that we were ready to accede to the claim that an hypothesis may be well confirmed, yet as long as it did not receive other kinds of support as well—the nature of which Mackie did not set out to specify—it may not be credible to any degree. Be that as it may, it will still be agreed by everyone irrespective of what criterion of confirmation they may be subscribing to, that the minimal function of such a criterion is to separate hypotheses into two classes: those which are confirmed by the evidence, to some degree, and those which are not confirmed at all. In other words, confirmation, even if not sufficient in itself to render an hypothesis credible to any degree, must not be turned into a completely vacuous concept. It may be taken for granted, therefore, that no criterion meets even the most basic requirement of adequacy if it confers confirmation of whatever degree upon *any* hypothesis on the basis of arbitrary evidence. Now let 'b' stand for any observation (not implied by k) then $p(b/k)<1$. Let h be any hypothesis and $H = h\&(h\supset b)$. Then since H implies b, $p(b/H\&k) = 1$ and $p(b/H\&k)>p(b/k)$. Thus, by Mackie's criterion C, b confirms H. But H implies h, hence h is confirmed at least indirectly by b. Thus, by C, every hypothesis is confirmed at least indirectly by random evidence. This deprives the notion of confirmation of all significance.

It is obvious, therefore, that even Professor Mackie's uncommon philosophical insight has not been a sufficient safeguard against the universal temptation to think that the relationship between b and H may determine whether b confirms H. The fact is that matters are somewhat more complicated, for it is the relationship between b and H *together* with the way H compares with all other statements that have the same relationship to b as H, which determines whether b confirms H to *any* degree. Mackie's basic criterion C is, therefore, obviously inadequate for it is not sufficient that $p(b/H\,.\,k) > p(b/k)$, in order that b should confirm H in relation to k. The *prima facie* case for thinking that b supports H is because H implies b, but then so does H' where $H' = \sim h\,.\,(\sim h \supset b)$. H is not simpler and is not, in any other way, superior to H'. In fact, of all the statements which imply b and no other observation statements, b is the simplest and, consequently, neither H nor H' receives any confirmation through b. We must conclude, therefore, the principle (of simplicity) which adjudicates among the various hypotheses which are similarly related to b, does not operate on its own; it determines which of these hypotheses is confirmed by b.

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MR GIBSON ON RAVENS AND RELEVANCE

In 'Relevance and the Ravens' (*Brit. J. Phil. Sci.* **18** (1967)), Mr Hooker and I examined the most popular solution to the paradox of the ravens; and as a preliminary to criticism of it, we set out, more explicitly than anyone had done before, the propositions on which that solution must rest. Since we claimed to show that together these propositions entail some false consequences, it would have been evident to any reader of our article that we thought that at least one of those propositions must be false. Mr L. Gibson, however, in his discussion of our article (*Brit. J. Phil. Sci.* **20**, p. 76), advances criticisms of some of those propositions which, if they were just, would convict this solution to the paradox, not of falsity in some of its less obvious consequences, but of obvious contradiction at its starting point.

But they are not just. Mr Gibson's criticisms, in fact, all stem from one mistake. This is, that he misreads various *statements of logical probability* in our article as though they were *statements of relative frequency*. What were, very explicitly, assessments (numerical or comparative) of "probability$_1$", he insists on treating as assessments of "probability$_2$". (See Carnap (1962), *Logical Foundations of Probability*, pp. 29-36.))

Here are two examples to prove that this is what has happened.

(1) ('*R*' and '*B*' stood for 'raven' and 'black' respectively, and '*k*' stood for the statement that a proportion x of things are ravens and a proportion y of things are black, where $0<x<y<1/2$.) The most popular solution to the paradox, Hooker and I said, rests on, among other things, the comparative statement of logical probability: (*c*) $Pr(Ba,k.Ra) = (PrBa,k)$. Mr Gibson says that (*c*) is "just another way of expressing" the proposition that "the proportion of black ravens in the class of ravens is the same as the proportion of black objects in the class of all objects"!

(2) The (obviously false) statement of logical probability, $Pr(Ba,k.Ra) = 1$, is said by Mr Gibson to be "equivalent to" the proposition that all ravens are black!

That there *is* a logical, non-factual, concept of probability, Hooker and I took for granted; as we were, surely, entitled to do. That in our article we were concerned with that concept of probability, and with no other, we made as explicit as possible. It is hard to see, therefore, how Mr Gibson can have misunderstood us so completely.

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A SUGGESTED RESOLUTION OF MILLER'S PARADOX

The purpose of this note is to argue that the paradox of Miller (1968) is neither as shallow nor as deep as it might appear, to give a proposed resolution of it, and to correct a criticism of it which I previously published at a time when I had underestimated it.

In a nutshell, one form of Miller's paradox can be expressed thus:

> 'For each x, $x = \mathrm{P}(E|\mathrm{P}(E) = x)$. Put $x = \mathrm{P}(\bar{E})$, where the vinculum denotes negation, and we obtain
>
> $$\mathrm{P}(\bar{E}) = \mathrm{P}(E|\mathrm{P}(E) = \mathrm{P}(\bar{E})) = \mathrm{P}(E|\mathrm{P}(E) = 1/2) = 1/2,$$
>
> so that the probability of any event E is $1/2$.'

In this form the paradox can be attacked on the grounds that it is dangerous to allow the propositions that are "given" (to the right of the vertical stroke) to make explicit reference to probabilities (Koopman, 1940; Good, 1950) since this skims too closely to well-known logical paradoxes. The most dubious equation in the string is the first one.

The second form of the paradox cannot be dealt with in this manner, since now the "given" information is not probabilistic. This second form can be expressed as follows:

> 'For any x, $x = \mathrm{P}(E|s = x)$, where s denotes (is an abbreviation for) "the long-run limiting relative frequency of 'successes' in a sequence of independent trials". If now we write for x "the long-run relative frequency of 'failures'", we can infer that all probabilities are equal to $1/2$.'

In order to resolve this form of the paradox it is convenient first to write down a theorem in the theory of probability that resembles the formula $x = \mathrm{P}(E|s = x)$ as closely as possible. Such a theorem is given by a form of the law of large numbers in the theory of intuitive probability, that is, logical or subjective probability. The theorem is that $\mathrm{P}(E|s = x) = x$, *where x is an assigned* *("given", or, for subjective probability, "known") real number between 0 and 1 at which the initial intuitive probability density of the physical probability of E is positive, and where the symbol* 'P' *refers to intuitive probability.* (Compare the 'narrow rule of induction' of Miller, 1968, which he regarded as unsatisfactory but which is as much as I am claiming is provable.)

In this theorem, if we try to put $x = P(\bar{E})$, then x is no longer an *assigned* real number and the conditions of the theorem break down. To make the substitution would be somewhat analogous to a refusal to regard epsilon as an assigned number in the definition of a limit. Other analogies would be statements such as '$f(n)$ tends to $1-f(n)$ as n tends to infinity' and '$f(n)$ tends to $f(n)$ as n tends to infinity'.

We now see why Miller's argument appears paradoxical. The formula $x = P(E|s = x)$ has enough of the truth to it so that it seems reasonable at first to regard it as a universal truth, perhaps even of the status of an axiom, whereas in reality it is only an approximation to a valid theorem of a familiar theory of intuitive probability. This theorem does not justify the substitution of $P(\bar{E})$ for x, and so no contradiction arises in this theory of probability. It is possible of course that a contradiction might arise in some other theory of probability, and if so that theory of probability would be refuted.

In a previous paper (Good, 1966) I argued incorrectly as follows:

> 'The statement $P(\bar{E}) = P(E)$ is impossible unless $P(E) = 1/2$, and this rules out the use of the expression $P(E|P(E) = P(\bar{E}))$.'

Miller (1969) pointed out that $P(E) = P(\bar{E})$, on the right of the vertical stroke, is merely *false*, and not impossible unless it is *known* that $P(E) \neq 1/2$. All sensible theories of probability permit false propositions to be "given". He is right about this since otherwise the product axiom of probability would be useless. He mentions also that Popper's system allows even impossible propositions to be "given" in order that ordinary logic should be subsumed under the calculus of probability.

An algebraic analogue of Miller's paradox was pointed out by Mackie (1966). It can be expressed succinctly thus:

$$1/2 = (\text{The value of } a \text{ if } a = 1/2) = (\text{The value of } a \text{ if } a = 1-a) = 1-a.$$

Here the fallacy is in the last step: an expression involving a cannot be legitimately described as the "value" of a. This algebraic paradox is analogous to the first form of Miller's paradox.

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PRZEŁĘCKI, Marian (1969) *The Logic of Empirical Theories*. London: Routledge & Kegan Paul. 30*s*. Pp. v+108.

For the past ten years, or so, the members of a group of Polish philosophers and logicians have been applying the methods of formal semantics (model theory) to the study of problems in the philosophy of science. The formal background to this approach is found in the works of A. Tarski and other mathematicians such as J. Łoś, J. G. Kemeny, and W. Craig; a large part of its philosophical background being provided by the writings of R. Carnap and especially K. Ajdukiewicz. Works by R. Suszko and A. Grzegorczyk have been influential in promoting the combination of these two streams of ideas, whose other notable exponents include R. Wójcicki and M. Przełęcki. Until recently, however, none of this research has been published outside Poland and little has been available in English. Marian Przełęcki's volume in the Routledge & Kegan Paul series of Monographs in Modern Logic is therefore particularly welcome. It surveys the well established common basis of the model-theoretic approach as well as contributing a substantial amount of new material. The general unfamiliarity with the approach and its technical nature make it necessary to provide a detailed exposition of the basic ideas before passing judgment.

In his preface the author points out that problems of syntax and semantics by no means exhaust the problems of the logic of empirical theories; yet "the logical syntax and semantics of the language of empirical theories may be regarded as a necessary prerequisite of any logical analysis of empirical science". Attention is focused on those features of empirical theories which distinguish them from mathematical ones, i.e. their semantics. But the same methods of analysis which have proved fruitful in connection with mathematical theories, namely the methods of model theory, will be applied to the analysis of empirical theories.

To this end, and to avoid technical difficulties, the languages considered in the monograph are elementary formalised languages with first-order predicate calculus as logical basis. By a *theory* formulated in such a language is understood a set of statements of the language, namely its *theorems*. Assuming this set to be a system closed under consequence and therefore infinite, and that most, if not all, interesting empirical theories are undecidable and therefore cannot be characterised by decision procedures, the only remaining means of an adequate and precise characterisation is *axiomatisation*.

Turning to semantics, the concept of a *model* of a formalised language and the related semantical concepts of truth, denotation, logical truth, consequence, etc., are introduced in the usual way. A formalised language equipped with a uniquely determined model is called *semantically determinate*. It is conjectured, however, that a large class of languages, including those of all empirical theories, fail to satisfy this condition. For such a language the factors which contribute towards the specification of its intended model—to a large extent non-logical in character—fail to determine it uniquely, succeeding only in delimiting a *family* of models. But it is suggested that if the family satisfies certain further conditions the resulting ***semantically indeterminate*** language might well be, and usually is, regarded as an interpreted meaningful language.

For semantically indeterminate languages the semantical concepts, especially those of truth and denotation, need further analysis. Three approaches are presented of which the second is of particular interest. On this approach a sentence α of L is true, when L is interpreted by the family M^*, if and only if α is true in each of the models in M^*. (Truth in a single model is understood in the usual sense.) α is false if and only if it is false in each of the models in M^*. Sentences of L which are either true or false in these senses are called ***determinate;*** those which are neither, ***indeterminate.*** It follows that when the class of indeterminate sentences is non-empty, as will usually be the case, the metalogical Law of the Excluded Middle fails to hold. In a corresponding way the denotation of a one-place predicate P_i may be understood to be the class of those individuals which, in each model in M^*, belong to the denotation assigned to P_i in that model (denotation in a single model being understood in the usual sense). Individuals which are included in the denotation of P_i in some models in M^* and are excluded from it in others are said to belong to the ***area of indeterminateness*** of P_i. A natural generalisation takes account of k-place predicates.

Chapter 4 is concerned with semantical questions more specific to empirical languages. Two ways in which a language may be interpreted are distinguished: a ***verbal*** and a ***non-verbal*** way. A verbal interpretation of a language L consists in specifying its family of intended models as those in which a certain set of sentences of L are true. The requirement that these sentences—called ***meaning postulates***—be true constitutes part at least of the intended interpretation of L. By an application of the Isomorphism Theorem it is shown that no language interpreted in a verbal way alone can qualify as a language suitable for an empirical theory. Even if this verbal interpretation is supplemented by the non-verbal determination of a unique universe of discourse U for the language L, this is still insufficient to guarantee L an empirical character. For the Isomorphism Theorem assures that each predicate P_i of a language interpreted in this way will either denote a ***logical relation*** on U or will be ***completely vague*** in U, in the sense that for any object x of U there will always be among the models of the interpreting family one in which x is included in the denotation of P_i in that model and another in which x is excluded from the denotation of P_i in that model. (A logical relation on a set U is one which is invariant under all automorphisms of U.) Thus the question whether a given individual of U, or more generally a given k-tuple of individuals of U, falls under a predicate is essentially undecidable for all predicates that are not constrained by the meaning postulates to denote logical relations.

Przełęcki concludes that if L is to possess an empirical character its interpretation must be provided by determining in a non-verbal way not only its universe of discourse but also the denotations of some of its predicates.

> If L is to be a language of empirical theory, the question whether an object x falls under a predicate P_i must—at least for some objects and predicates—be decidable on the basis of experience. This seems to determine the character of the non-verbal interpretation of P_i. P_i has to be interpreted

> *ostensively*, that is, roughly speaking, by pointing out the objects it applies to (p. 32).

It is assumed then that among the predicates of L there are some which have been interpreted ostensively, and furthermore *purely* ostensively, without additional verbal procedures.

> The assumption that every empirical theory employs some observational terms of the kind described is by no means indisputable, and, in fact, will be questioned by us later. In this context, however, we shall, for simplicity's sake, accept this assumption without any further discussion (p. 34).

Terms satisfying this assumption are called *observational*, or simply *O-terms*, and the sublanguage of L containing just these predicates as descriptive constants is called the *observational*, or *O-language*, L_0.

In Chapter 5 the procedure of ostensive definition of O-terms is examined more closely. The author admits that "this appears to be a somewhat enigmatic procedure which is certainly in need of a thorough examination" (p. 35). It is suggested, however, that a characteristic feature of an ostensively defined predicate is its *vagueness*: there will always exist things such that the question whether it applies to them is essentially undecidable. This is represented in the reconstruction as follows. It is assumed that within the domain of *observable* physical objects U_0 the ostensive interpretation of the terms of L_0 provides L_0 with a *family* of intended models M_0 each with domain U_0. (How such an interpretation is achieved in practice presents a problem for a psychologist rather than a logician.) When the extension is made to the domain U of all physical objects—including *unobservable* ones as well—it is assumed that within $U-U_0$ (the domain of unobservable physical objects) any ostensively defined predicate is completely vague, i.e. each such object belongs to the area of indeterminateness of the predicate. In this second case L_0 is understood to be interpreted by a family of intended models M_0^* defined as the family of models of L_0 which (i) have as universe the set U and (ii) are *extensions* (in the model-theoretic sense) of some member of the family M_0.

Chapter 6 is concerned with the interpretation of theoretical terms (*T-terms*). These are just the terms of L that are not in the vocabulary of L_0; they therefore receive no direct interpretation. Instead they are interpreted by a set of meaning postulates MP which connect them with the terms of the previously interpreted sublanguage L_0. T-terms are to be interpreted in such a way that the meaning postulates, in which O-terms retain their preassigned interpretations, are true. In this way L is provided with a family of intended models M^* consisting of just those models of L in which (i) O-terms receive the denotations provided by some member of M_0^* and (ii) the meaning postulates MP are true.

The question arises concerning the nature of the set MP. Can it be any set of sentences of L or must it satisfy certain special conditions? The fact that the languages of empirical theories seem to be treated by scientists as interpreted meaningful languages—and this independently of any empirical findings—suggests that MP must meet the *semantical condition of non-creativity*. This condition states in effect that M^* as defined above is to be non-empty no matter what family of intended models L_0 may have been assigned, i.e. independently of the contingent constitution of M_0^*. Thus, roughly speaking, the set MP

is non-creative (in the semantical sense) if, no matter what interpretation is given to *O*-terms, its members can all be made true by suitable interpretations of *T*-terms within the given universe. If *MP* entails no non-tautological sentence of L_o—if *MP* is non-creative in the *syntactical* sense—this condition is clearly satisfied. (The converse fails to hold.)

The set *MP* of meaning postulates for *T*-terms is to be a semantically non-creative set of sentences of *L*. But which one? "In answering this question", Przełęcki writes (p. 54), "we clearly have to resort to some pragmatic factors, in particular, intentions and decisions of scientists who have been constructing a given language and theory." It is supposed that a set *P* of *postulates* for *T*-terms has been determined by the decision of the users of the language to understand *T*-terms in such a way that the sentences of *P* be true "as far as this proves possible". But if *P* fails to satisfy the condition of non-creativity the whole set *P* cannot be identified with the set of meaning postulates *MP*. In this case *P* appears to contain, besides meaning postulates, some factual statements as well. The task remains of isolating the set *MP* from the whole set *P*.

Two conditions are imposed on *MP*. The first is the condition of non-creativity mentioned above. *MP* must be sufficiently weak to satisfy this condition. On the other hand it must also be sufficiently strong to include all of the meaning postulates "contained" in *P*; it must in a certain sense "correspond" to *P*. The second condition is intended to ensure this. It has two components. The first requires that the members of *MP* be logical consequences of the set of postulates *P*: they are therefore meaning postulates "contained" in *P*. The second component, whose precise content cannot profitably be summarised here, attempts to ensure that *MP* exhausts all such meaning postulates. These two conditions, however, fail to guarantee the existence or uniqueness of a set of statements satisfying them. In cases where such a set exists—and these appear to be the most typical—they allow scope for the choice of a set *MP* which reflects the specific intentions of the scientist engaged in constructing the given language and theory.

As regards the further characterisation of the set of postulates *P*, it is assumed to be a subset of the set of axioms of the theory *T* (where *T* is understood to include any logically prior theory in which some *T*-terms of *T* have been defined). It follows that each member of *MP* is a theorem of *T*. Often a theory is presented without any explicit distinction between its postulates and factual hypotheses. In such a case the meanings of *T*-terms seem to be dependent on the whole set of axioms of *T* and it is natural to identify the postulates with the members of this set. But this is a clear case where the set of meaning postulates cannot be indentified with the set of postulates, i.e. with the whole set of axioms of *T*, if *T* is to be an empirical falsifiable theory. The axioms of the theory then fulfil two functions; they both assign meanings to the theoretical terms and, at the same time, express factual hypotheses. They may be said to be composed of a "definitional" and a "factual" component.

In Chapters 7 and 8 detailed attention is given to the kinds of statement that are often held to function as postulates for *T*-terms. These include explicit definitions and reduction sentences of various sorts.

Chapter 9 contains a discussion of the main types of statement to be found in the language of an empirical theory. *Analytic* sentences are identified with the logical consequences of the set of meaning postulates. It is shown that the truth of these, and only these, can be established independently of any empirical findings. A sentence is called *contradictory* when its negation is analytic; the remaining sentences are called *synthetic*. Analytic and contradictory sentences are all determinate. But for most semantically indeterminate languages the class DT of determinate sentences fails to exhaust the class of all sentences. Since the composition of DT cannot be established without empirical investigation, it is suggested that it can hardly be identified with the class of empirically meaningful sentences. Two further concepts of determinacy are introduced to avoid this difficulty, the weaker being proposed as an explication of the concept of empirical meaningfulness.

According to the account outlined above the language of an empirical theory may be thought of as resulting from a single extension of the observational language. In practice, however, theoretical terms often seem to be introduced into previously interpreted languages which themselves contain theoretical terms with interpretations that are to be preserved under the extension. Chapter 10 shows how this situation can be accounted for within the framework established. It also shows how the reconstruction might be modified to meet the criticism that no scientific language contains any purely ostensive predicate.

In his final chapter the author points out that the treatment of empirical theories presented in the monograph has abstracted from any process of change and development. An actual scientific theory is constantly changing both in respect of its set of theorems and in respect of their logical status. What is treated as a single—but vaguely defined—theory in scientific practice must be treated as a family of precisely defined theories in logical considerations. Each member of the family can be seen as a logical "cross-section" of the theory of scientific practice. Przełęcki concludes with a biological analogy:

> Logical reconstruction of a scientific theory is like making 'slices' of a living organism. This certainly distorts our original object of inquiry. But only then can it be put under a logical microscope (p. 106).

The empiricist nature of the reconstruction presented is clear. Not surprisingly two of its essential components are the observational-theoretical and analytic-synthetic distinctions. Przełęcki seems to be well aware of their many current criticisms. To take account of those of the first, he suggests in Chapter 10—entitled "Towards a more realistic account"—that the observational language L_0, whose only predicates are purely ostensive and therefore logically independent, might be seen as "a useful epistemological fiction, which helps us to account for the kind of interpretation characteristic of an empirical language" (p. 102). But it should be remembered that the claim that the meanings of all terms of a theory are determined by the theory as a whole—a claim sometimes made in criticism of the observational-theoretical distinction—if understood as meaning that its interpretation is purely verbal in the sense of the monograph, issues in an extreme form of conventionalism (as is shown in Chapter 4). An empiricist is therefore committed to the existence of a certain subclass of the terms of an empirical theory whose interpretation is provided, to some degree, in a direct non-verbal way independently of the theory. It is sufficient to take these to be the "observational" terms of the reconstruction. The extent to which they can sustain analytical interdependencies compatibly with an empiricist account of science is one of the interesting problems suggested by the monograph.

The central feature of most current criticisms of the analytic-synthetic distinction seems to be its vagueness in connection with natural discourse, scientific or otherwise. Przełęcki does not deny this. He admits, citing Newtonian mechanics as an example, that there may be available no definite answer to the question which of the axioms of a theory are postulates endowing their terms with meaning and which are factual hypotheses, if the answer is to be based on the actual behaviour of scientists. This means that scientific theories can be reconstructed in a variety of ways none of which can be conclusively refuted by an appeal to scientific practice. Furthermore it seems to the reviewer that for a typically philosophical analysis of science, rather than a psychological or sociological analysis of the beliefs or behaviour of scientists, idealised reconstructions of the subject matter of the enquiry are not merely desirable but unavoidable. Whether or not they should be of a formal nature will depend on the problems under discussion. But differing reconstructions may well fail to be neutral with respect to competing philosophical views of science and there may be available no generally accepted empirical criteria for deciding their relative degrees of "realism". In such cases it seems most natural to regard the differing reconstructions as explications of their associated families of irrefutable, though criticisable, philosophical doctrines. In this light the monograph under review will be seen as an explication of certain versions of empiricism.

One of its most valuable features is the wide range of problems suggested for further study. An important one concerns the further characterisation of the class of analytic statements of a theoretical language. This problem is left open in the monograph to the extent allowed by the partial definition of the set *MP* by the two conditions mentioned above. But it is fairly clear how a precise distinction between the analytic and synthetic can be made; one which furthermore respects the intuitions of those who believe that the axioms of most theories (as they are normally formulated) play a dual role.[1] These conditions, however, allow for a choice of the set *MP* in accordance with which the extensions of *T*-terms in theories with some (determinate) false observational consequence are wholly undetermined within the given universe. The question of the interpretation of *T*-terms in false theories seems to deserve further consideration.

Another problem concerns the universe of discourse of a theoretical language. In the monograph it is supposed that the universe—"the set of all physical things"—is somehow given, and that the meaning postulates corresponding to the theory then provide an interpretation for *T*-terms within this universe. In practice, however, it seems that the scientist's ontology is extended to meet the demands of theory (as far as this proves possible) rather than being given in advance. It is an interesting question—which can only be precisely formulated and answered with the help of model theory—to ask what extensions are required in a previously accepted universe, i.e. what novel theoretical entities must be admitted, if first the theory is to be regarded as interpreted and secondly it is to be regarded as true (when this proves possible).

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[1] Various definite solutions to the problem are reviewed in PRZEŁĘCKI, M. & WÓJCICKI, R. (1969) The problem of analyticity. *Synthese* 19, 374–99.

This is an example of a problem of change. The model-theoretic approach provides an eminently suitable framework for discussing logical aspects of this sort of problem. It is to be hoped that Chapter 10 of the monograph and the article by Roman Suszko cited in the bibliography,[1] will help dispel the widespread misapprehension that formal methods are inapplicable to the study of processes of change and development.

The monograph also provides a useful basis for clarifying the realist-instrumentalist dispute; the controversy is not adjudicated though certain extreme forms of either position seem to be ruled out. The semantical (model-theoretic) framework also provides a natural setting for discussions of the problem of empirical meaningfulness. An explication of at least one sense of this ambiguous notion is provided. An interesting feature of the languages considered in the monograph, which bears on both these questions, is their indeterminacy. The problem of the definition of truth for indeterminate languages is of considerable interest. Perhaps the most attractive of the definitions proposed is such that the metalogical Law of the Excluded Middle fails while the logical law is maintained. Such a concept of truth, however, fails to conform without qualification to the classical equivalence: '*p*' is true if and only if *p*. This suggests that the question of the criteria of adequacy for the definition of truth in indeterminate languages needs careful examination. However this may be, an indeterminate sentence (q.v. above), being neither analytic nor contradictory, is classified as synthetic; but the author expresses a doubt whether it "might qualify as a sentence whose truth-value depends on experience"[2] (p. 92), this qualification being reserved for determinate synthetic sentences. Yet not all empirically meaningful sentences need be determinate if it is accepted with the author that all determinate sentences are empirically meaningful, but that the class of empirically meaningful sentences cannot be identified with the class of determinate sentences on account of the contingent nature of the latter's composition. It seems, however, that whichever of the suggested definitions of truth is adopted, an indeterminate sentence cannot be known either to be true or false (possibly because it is neither). If so, the epistemological categories *a priori* and *a posteriori* fail to exhaust the class of meaningful sentences, with the consequence that the logical empiricist tenet that all meaningful sentences are either analytic *a priori* or synthetic *a posteriori* cannot be maintained for the proposed reconstruction without qualification. (The non-existence of synthetic *a priori* and analytic *a posteriori* sentences is guaranteed independently, however, by the proposal to understand analytic sentences as just the logical consequences of the set *MP* and the condition of non-creativity imposed on that set, respectively.)

The monograph is a welcome addition to the literature on the formal approach to the philosophy of science. It will be especially welcomed by those interested in the model-theoretic approach, being the first contribution to the literature in this form. A slight acquaintance with elementary formal syntax is presupposed but all specifically model-theoretic concepts are introduced by definition, though sometimes these are of an informal nature—in the case of truth for example—on account of the limits imposed by the form of the work. It makes a timely appearance in view of the present interest in syntactical and semantical problems of scientific theories—notably problems concerned with "incompatibility" and "meaning"—and of current criticisms of empiricism. A more widespread familiarity with the rigour, precision, methods, and results of the approach presented might have a beneficial effect on contributions to the debate.

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[1] Suszko, R. (1968) Formal logic and the development of knowledge. *Problems in the Philosophy of Science*. Ed. I. Lakatos and A. Musgrave, Amsterdam: North-Holland Publishing Company, pp. 210–30.

[2] The psychologistic flavour of this phrase is unfortunate. It seems that the expression 'depends on experience', which occurs frequently, could be replaced throughout by 'depends on matters of fact' or 'is contingent' without doing violence to the author's intentions.

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van PEURSEN, C. A. (1969). *Leibniz*. (Trans. by Hubert Hoskins). London: Faber & Faber. £1. 50 (30*s*.) Pp. 128.
SIMONOVITS, Anna (1968). *Dialektisches Denken in der Philosophie von Gottfried Wilhelm Leibniz*. (trans. by W. Schuffenhauer, A. Noelle & M. Faak). Budapest: Akadémiai Kiadó. £1.75 (35*s*.). Pp. 239.

Professor van Peursen's book opens with an outline of Leibniz's life and teaching, in which he rightly stresses Leibniz's persistent desire to reconcile opposites in a wide variety of fields. The book concludes with a chapter purporting to show the relevance of Leibniz's philosophy today, but which is no more than an outline of later developments of three Leibnizian topics: logic, the question 'Why is there something rather than nothing?', and cosmic evolution. More useful would have been an account of how Leibniz's philosophy is to be understood against the background of his predecessors and contemporaries.

The actual exposition of his philosophy occupies the intervening three chapters—a mere 70 pages. It is a great relief to find a book on Leibniz which is not organised on the supposition that his philosophy was a sort of logical calculus; but it is hard to detect any organisation at all in van Peursen's exposition. The first of these chapters, 'The Inside of Reality: the Monads', contains little about monads beyond the assertion that they are logical subjects (p. 38), symbols (p. 43), rules (p. 44), and original forces (p. 45) (all highly misleading characterisations—that they are substances is barely mentioned), and there is much more space devoted to logic, causality, appetition and other topics. The second, 'The Outside of Reality: Organization, Evolution, Perception, Harmony', covers roughly what it claims to; and the third, 'The Logic of an Optimal World: Truth, Freedom, God', is rather about logic, freedom, and the principle of perfection.

Presumably, the rationale behind this apparently haphazard distribution of topics lies in the distinction between the "inside" and "outside" of reality ('interiority' and 'exteriority'). Indeed, an understanding of this distinction is presupposed throughout the book, and it is remarkable that so vague a metaphor, so central to the content and organisation of the work, should escape without any sort of explanation. One of the most fundamental questions of Leibnizian exegesis is that of the relation between monads and the material world. According to van Peursen, the former are the "inside", the latter the "outside" of reality; but to say this and no more is simply to obscure the question and not to answer it. Indeed, he never so much as hints that there is a problem here. In Leibniz's writings there is an apparent conflict between two characterisations of the status of the material world: sometimes he says it is purely phenomenal, at others that it is an aggregate of monads. This gives rise to a fundamental difficulty of interpretation, which a commentator should point out and try to resolve; but van Peursen merely follows Leibniz into the same ambivalence. On page 32 he contrasts immaterial monads with "the spatial world of phenomena" and "the material and exterior aspect of things" (so the "outside" of reality is phenomenal); but on page 62 we find: '... if in line with Leibniz' metaphysics generally we take 'outside' as referring to what cannot be accounted for on the basis of the individual monad but only in terms of each monad's insertion into the logico-synchronic system of '*harmonie préétablie*' ...', and here the contrast is between the individual monad, and a harmonious multiplicity of monads. Elsewhere (p. 89) it is drawn between the external world and the interiority of the monad. But the external world *is* monads, so what is the distinction supposed to be? Perhaps it is meant to be between the individual monad considered in itself, and considered in relation to all other monads. But what is a monad apart from its relation to other monads? *Materia prima*, perhaps?—but van Peursen never even mentions it. Since it is difficult to see how such a contrast between the "inside" and "outside" of reality is to be applied in the case of Leibniz's philosophy, it is hardly surprising that the distribution of topics between the two chapters appears highly arbitrary.

The above is a good example of my main criticism of the book: that it never goes deep enough to be giving one interpretation rather than another. Most of the philosophical topics discussed by Leibniz are treated in enough detail to give a vague idea of his views, but no more. A number of subjects are not even touched on, for example primary matter and form, space, atomism, the creation of the world, and ethics. Other topics are merely mentioned, without any discussion or explanation; for instance on page 65 he refers to the *vinculum substantiale*—"the substantial link between substances, and specifically between soul and body". But why bother to mention it at all if nothing is to be said about what it is, and what role it plays? According to Russell, it is "rather the concession of a diplomatist than the creed of a philosopher" [1]; according to Boehm, he was hesitant about it [2]; according to Rescher, it is equivalent to the presence of a dominant monad [3]; and according to Blondel, it is the keystone of his philosophy [4, *passim.*]. Where does van Peursen stand?

If all that is wanted is a rough sketch of Leibniz's views, then this book is adequate, and less harmful than many other introductions, since in virtue of giving no interpretation, it avoids a wrong one. But for anyone who intends to give the subject a serious study, it is simply too unscholarly (it even gives no references) and too superficial to be of any use.

By contrast, Anna Simonovits's book is relatively thoroughgoing and scholarly, intelligently organised, and not lacking in useful background information. The author is a dialectical materialist, and this colours her approach in at least two ways, though not to the extent one might expect.

First, there are a number of peculiarly Marxist explanations of why Leibniz believed what he did, in terms of his social and economic environment. For instance, she ascribes his rejection of materialism to German feudalism (p. 42); she remarks that his doctrine of monadic domination is the philosophical reflection of contemporary capitalist social structure (p. 62); she describes his conception of freedom as characteristically bourgeois (p. 140); she excuses his idealism on the grounds that social and scientific conditions were not ripe for a materialistic dialectic till the nineteenth century (p. 170); and she relates his interest in the individual to the growth of capitalism (p. 180). However, these and similar comments supplement rather than replace philosophically more relevant explanations. Perhaps some of the history in the book is bad history, but at least it is history—and this is far healthier than the all too common attitude that philosophies of the past may be treated as timeless.

Second, the author's avowed purpose is to bring out the extent to which Leibniz's philosophy reveals dialectical tendencies—that is, to show how far it is *like* dialectical materialism. But the analogies she produces are not so much doctrinal as methodological: Leibniz's philosophy is dialectical inasmuch as it brings about a synthesis of opposites, whatever the content of that synthesis may be. However, it is so easy to express almost any philosophical position as such a synthesis, or reconciliation of conflicting views, that her thesis verges on the trivial. At best she can be said to have shown in detail that Leibniz was unusually concerned to find a middle way between extremes. This is very true, but it is unfortunate that she feels it necessary to prove the point by translating his views into the cumbersome phraseology of dialectical materialism. Her dialectical standpoint does not give rise to seriously biased interpretation (far more serious is, e.g. Russell's logical bias); rather it serves as a substitute for interpretation. Apart from reporting on what a philosopher said, the commentator should explain it, and make it more comprehensible to his readers; but we find little of this in Simonovits's book—instead, she confines herself to a comparison of what she has reported with the most general features of dialectical materialism.

As for her exposition of Leibniz's philosophy, it is quite comprehensive and well supplied with quotations and references, though there are some serious inaccuracies. For example, she says (pp. 68, 114) that monads are originally identical and only develop differently—an assertion which is not merely unfounded, but in direct conflict with Leibniz's view that monads are pregnant with their future states. Again, on page 71, she says that for Leibniz the world is a unity, the Monad of Monads; but this is a doctrine which he explicitly rejected (that the world is not a unity was his main argument against the view that God is the soul of the world).

As a supplement to the book, the author has added a 15-page review of the more important commentaries on Leibniz. This is a useful practice which could well be adopted more often.

REFERENCES

[1] Russell, Bertrand (2nd. edn., 1937) *A Critical Exposition of the Philosophy of Leibniz.* London, p. 152.

[2] BOEHM, A. (1938) *Le 'Vinculum Substantiale' chez Leibniz*. Paris, p. 113.
[3] RESCHER, Nicholas (1967) *The Philosophy of Leibniz*. Englewood Cliffs: Prentice-Hall, p. 121.
[4] BLONDEL, Maurice (1930) *Le 'Vinculum Substantiale' d'après Leibniz*, Paris.

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SANTILLANA, Giorgio de (1968) *Reflections on Men and Ideas*. Cambridge, Mass.: The M.I.T. Press. £7.00 (140*s*.) Pp. xiii+381.

Reflections on Men and Ideas brings together twenty-four of Santillana's essays and lectures, of which all but two, a course lecture on Newton, and 'Paralipomena of the Future', have been in print for some years now. The virtue of having them in one volume lies in the opportunity to discern the themes that unite Santillana's far-ranging excursions into the cultural history of science.

From the variety of topics treated in these essays—pre-Socratic thought, primitive cosmology, Renaissance art and science, the Enlightenment, modern physics, and the philosophy of science—four such themes emerge to characterise the author's thought. One is the case of Galileo and its implications. Galileo has become Santillana's touchstone for viewing those points at which culture and science come into conflict. As a revolutionary rationalist, prepared to assume "responsibility for first and last things", Galileo marks for Santillana the coming of age of science. The inevitable conflict that arose when the scientist tried to move beyond the traditional limits of his discipline to accept such responsibility has its modern parallel in the case of J. Robert Oppenheimer ('Galileo and Oppenheimer').

The essay 'Scientific Rationalism' itself forms a second link among these articles. Santillana freely admits his prejudice. He believes in this *Weltanschauung*, which he characterises as the search for a metaphysical unity in the physical world. By succumbing to such modern sirens as positivism and operationalism, science has not so much freed itself of metaphysics as it has surrendered metaphysics to the non-scientists, or even anti-scientists. The realm of human thought perhaps most concerned with "first and last things" has thus alienated itself from its culture, with the result that, "The mirror of nature that reason had endeavoured to build up through the ages is shattered, and we look for the first time straight out into an unknown world" ('The Seventeenth-Century Legacy: Our Mirror of Being').

Here one finds a curious humanist-philosopher schizophrenia in Santillana. The very line of thought that takes man out of some cosmic grandstand and forces him into the middle of the fray, making him again, in a sense, "the measure of all things", only offends the philosophical sensitivities of a man who otherwise so forcefully argues the humanity of the scientific enterprise.

Earlier generations of scientific thinkers knew enough to stay out of the fray. From its beginnings in the thought of the pre-Socratics, scientific rationalism served as home port for Western scientific imagination. Through the seventeenth century at least, Santillana sees thinkers reaching back to the always vital rationalism of the pre-Socratics in the search for the unifying thread of reality.

This continued vitality of pre-Socratic thought forms a third theme of Santillana's essays. In the new scientific attitudes of the Renaissance and seventeenth century he finds less of Plato than he does of Pythagoras and Parmenides. Their way of thinking about the world, indeed of visualising it, had become so ingrained in Western thought that even "Leonardo, Man Without Letters" finds an intellectual home therein. Brunelleschi's contribution to "The Role of Art in the Scientific Renaissance" lay in the application to architecture of "the unformulated Pythagorean element just then being transformed."

Santillana's approach to the pre-Socratics themselves, in particular to Parmenides, reflects this view of their later influence. It also reflects Santillana's characteristic pan-cultural view of history, but here in a field for which there are models and guidelines. His endeavours reach out to criticise the philologist's blind reliance on texts ('Philolaos in Limbo, or: What Happened to the Pythagoreans?'), whereby thinkers are drummed out of existence to save men like Diogenes Laertius from the oblivion their foolishness deserves. The highly entertaining 'Paralipomena of the Future' warns us of the ridiculous fate that awaits our time at the hands of future doxographers. Rather than relying on the garbled testimony of fourth-rate minds, Santillana insists on the wider view, the historian's view, that treats texts as merely one component of our knowledge of the past. In establishing the proper dates of Eudoxos, that "grey eminence" behind Euclid's *Elements*, Santillana puts a keen edge on this critical historical approach. But the 'Prologue to Parmenides,' in which Parmenides' Being becomes modern Euclidean space, contains far more of Riemann than one might care to see.

The pan-cultural approach to early Greek thought leads Santillana almost inexorably to "Les grandes doctrines cosmologiques" and beyond that to his latest two works on ancient myth and cosmology. His emphasis on the vitality of myth even in the "rational" systems of the pre-Socratics represents a welcome antidote to the literature that has Greek philosophy springing full-grown from the forehead of some rationalistic Zeus. But the critical reader will want to bear in mind the arguments of Frankfort and others for a real distinction separating mythopoeic from rational thought. Faith in scientific rationalism seems to skirt those arguments, for, in supposing that the human mind mirrors Being, "that what happens is conceivable", one may be tempted to conclude that the human mind can think in only one way, that only substantive conclusions or modes of expression change. Both are reasonable propositions, but both are debatable.

Detailed criticism of the individual articles would be out of place here; each has already run the scholarly gauntlet. What minor faults in fact and interpretation one may find pale before the brilliance of the whole. For *Reflections on Men and Ideas* sets the reader to reflecting. Hence, its value lies not only on the always provocative, always intriguing thoughts of the author, but also in the thought it stimulates in the mind of the reader. I can think of no higher praise for a work.

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Duhem, Pierre (1969). *To Save the Phenomena* (tr. by Edmund Doland and Chaninah Maschler, with an introductory essay by Stanley L. Jaki). Chicago and London: University of Chicago Press. $7.50. Pp. xxvi+120.

This is a welcome translation of Duhem's stimulating little work "ΣΩΖΕΙΝ ΤΑ ΦΑΙΝΟΜΕΝΑ: *Essai sur la notion de théorie physique de Platon à Galilée*", first published in 1908. It is an analysis of that case-history in scientific method which Duhem's own historical work has made classic: the interpretations and conflicts surrounding Aristotelian and Ptolemaic astronomy up to the condemnation of Galileo. The introductory essay strangely does not mention Duhem's *La théorie physique*, published two years previously, in which he presented the most complete account of his philosophy of science, but it is clear how several themes of that earlier work were fashioned by reflection on this astronomical case-history.

There is first the distinction between "metaphysics" and "physics", seen as concerned respectively with realistic explanation (which is *not* the aim of physics), and "saving the phenomena", or "mathematical representation" (which is). The distinction was made at length by Geminus and Simplicius (quoted on p. 10), and Duhem gives copious evidence from subsequent writers of many variations on the same theme. The apparent conflict between the Aristotelian geocentric universe of nested spheres and the Ptolemaic mathematics of epicycles or eccentrics was sometimes resolved by asserting that the geometric constructions cannot represent the real, and that since different hypotheses may equally save the phenomena, other constructions nearer to Aristotle should be sought. On the other hand some writers less wedded to Aristotle held that *no* knowledge of the heavens, even Aristotle's, could be perfect, and therefore we must live with uncertainties and perhaps contradictions with regard to real causes. Duhem's own favoured view is that of fourteenth and fifteenth century Paris, which rejected altogether the relevance of Aristotle for physics, defined as a science derived from experience with the sole purpose of saving the phenomena: "between 1300 and 1500 the University of Paris taught a doctrine of physical method which far surpassed in truth and profundity all that was going to be said on this subject until the middle of the nineteenth century" (p. 60).

It is interesting that another main theme of Duhem's philosophy of science is not illustrated or made explicit in the present work. This is the thesis that a single hypothesis is never refuted by experience but that only a whole group of hypotheses taken together can be so refuted. None of the writers represented here appears to have attempted to save the Aristotelian hypothesis by reinterpretation or rejection of other logically related hypotheses; on the contrary, certain aspects of the empirical data, the exactitude of predictions on the Ptolemaic system, the retrogressions, and the varying apparent diameters of the planets, are taken as conclusive evidence that the Aristotelian hypothesis is (astronomically and mathematically) false. Can it then be "metaphysically" true? There are here few of the puzzling overtones of *La théorie physique*, where Duhem suggests that we have access to metaphysical truth independently of scientific data, and that ultimately apparent contradictions between physics and metaphysics will be reconciled in the convergence of both towards one truth.

In the present work dogmatic Aristotelians and fundamentalist theologians are both taken to task for their excessive realism, along with Copernicus, Kepler and Galileo, whom Duhem claims to have been equally mistaken in their realistic interpretation of the heliocentric system. All alike should have followed the tradition of Proclus and Simplicius as did those like Osiander and Bellarmine who tried to reconcile the factions. But here both Duhem's quotations and his commentary vacillate confusingly between a proper wariness of the fallacy of affirming the consequent in regard to the truth of hypotheses, and the much stronger instrumentalist position according to which hypotheses have neither truth-value nor real reference.

In the closing pages the contribution of the heroes of the Copernican revolution is explained as a dim apprehension of the truth that the mathematical mechanics of heaven and earth must be unified. Duhem does not explain his conclusion that there was no factual content in their assertion that the earth moves, hinting only that their achievement would have been empty without Newton. But even if Newton's synthesis is taken to include relativity of linear motion, it is surely a weak foundation on which to build an instrumentalist interpretation of the system of the world. Newton himself asserts the real rotation of the earth, and as later became clear in the light of Mach's principle, any plausibility instrumentalism may have had in regard to relative linear acceleration also collapses. Galileo and the Inquisition were not shadow-boxing; *e pure se muove.*

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DEBUS, Allen G. (1968) *The English Paracelsians.* London: Oldbourne. £2.25 (45*s*.). Pp. 222.

Although Paracelsus was undoubtedly the dominant voice in Renaissance chemistry, there has always been widespread disagreement about his precise contribution to the science of this period. Indeed, estimates on the value of pre-Lavoisian chemistry vary enormously, its heroes withstanding scholarly scrutiny less well than the innovators in other experimental sciences. Close inspection indicates the extremely slow advance of experimental chemistry, which became only gradually divorced from a system of archaic and occult explanatory principles. In addition, chemistry was inextricably associated with alchemy and mystical speculation, making it an extremely difficult and somewhat uncongenial field for historians of science.

During recent years, Professor Debus has done much to rehabilitate the study of sixteenth century chemistry. He has shown that the Paracelsian chemists created a tradition of experimental chemistry, which provided an indispensible background to the chemists of Boyle's generation. Not daunted by the complex technical language of sixteenth-century chemistry, Debus has shown that there was an active interest in establishing an empirically verifiable science, the development of which was not seriously hindered by bizarre chemical theory.

As the comprehensive bibliographies indicate, the author has explored an impressive range of neglected vernacular chemical literature. The text considers a host of minor scientific and medical controversialists, as well as major thinkers like Robert Fludd who, until recently, has been inexcusably neglected by historians of science. Perhaps surprising omissions are Bacon and Dee, both of whom are directly relevant to the Paracelsian controversy. Debus also provides an insight into the scientific attitudes of the various medical organisations during this important phase of specialisation and professionalisation.

Two important themes are developed in the book. First, the century 1540–1640, involved gradual acceptance of chemical therapy, itself an important stimulus to controlled chemical experiment and new techniques of qualitative analysis. The author scrupulously avoids claiming that this important development was entirely the work of a partisan "Paracelsian" movement. Indeed he fully recognises the varied origins of chemical therapy, acknowledging that its acceptance was accelerated by the traditionally "Galenist" College of Physicians. Secondly, the author argues that the ready assimilation of chemical therapy contrasts with widespread suspicions about Paracelsus's general philosophy of nature. In particular Debus detects a general English distrust of the occult and magical. This particular theme needed to be discussed more fully; chemical therapy was defended by Paracelsians on the basis of an elaborate metaphysics, and the scientific and philosophical issues were associated by many of the critics of chemical therapy. Furthermore, as the popularity of Paracelsus, Helmont and Glauber in the period after 1640 indicates, complex, theologically orientated bodies of chemical theory continued to attract English readers. It may be that there was a delay in transmission of Paracelsus's elaborate speculative system, rather than active opposition to it before 1640. However, whatever the course, Debus nobly admits that no distinct Paracelsian school developed in England, to parallel the movements in Germany and France. Even more surprising, painstaking scholarship has failed to reveal the existence of a single unambiguously Paracelsian Englishman in the period under consideration. This may point to the rather diminutive role of Paracelsus in Debus's celebrated "Paracelsian compromise".

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Szabó, Árpád (1969) *Anfänge der griechischen Mathematik* München und Wien: R. Oldenbourg Verlag. D.M. 48. Pp. 494.

One need not read very far in this book to come to a judgment about it. It is a book that anyone who deals with ancient mathematics and philosophy must read and read carefully. Whether one will agree or disagree with Szabó's interpretations and substantive conclusions, one cannot ignore his arguments. Iconoclastic in tone and direction, these arguments are also thoughtful, provocative, and based on an intimate knowledge of the original sources and the secondary literature.

The book's iconoclastic spirit makes it at times difficult to read. For Szabó argues more than he narrates. As he himself warns in the Introduction, he offers not so much a history of the beginnings of Greek mathematics as a highly technical attempt to set details of that history straight. As such his work demands of the reader a knowledge of at least the broad outlines of pre-Euclidean mathematics. Szabó has, in short, written a scholar's book. The more one knows about the subject treated, the more one can gain from the author's insights.

Szabó came to his topic from the field of classical philology, his interest in early Greek mathematics having been aroused, he says, by his study of the Eleatics. Hence he brings to his investigation primarily the tools of the philologist. Words are Szabó's business, and the importance of words forms the core of his thesis. Greek mathematics had a technical vocabulary. In the early stages of development that technical vocabulary was borrowed from non-mathematical usage. The particular origin of a word and its original meaning throws light on its meaning as a mathematical term and on the exact concept it was intended to denote. Hence in many cases one can clarify the conceptual content of early mathematical passages—and therewith their contribution to historical understanding—by seeking the precise meaning of the terms employed in them. To his scholarly tools, Szabó adds a great deal of good sense, mathematical and otherwise.

In the first section of this tripartite work, Szabó grapples with the early history of incommensurability, using as a focus the famous passage of Plato's *Theaetetus* (147C–148B). In establishing the origin and exact meaning of the term *dynamei symmetros*, Szabó convincingly argues the antiquity of the problem of incommensurability as well as the likelihood that Plato was lauding less Theaetetus' originality as a mathematician than Theodorus' brilliance as a pedagogue. But the passage from Plato is only the focus of the first section. Surrounding it is a wealth of insight into the pre-Eudoxian treatment of incommensurable lengths.

Section II ostensibly deals with the theory of proportions, but in fact it is about music. For Szabó finds the origins of the technical vocabulary of ratios and proportions in Pythagorean investigations into the divisions of the monochord. These investigations eventually led to the purely mathematical problem of finding the mean proportional between any two magnitudes, and in turn to that of incommensurability. Where other scholars have found in Books II and VI of Euclid's *Elements* a "geometric algebra" of Babylonian origins, Szabó finds (in part) a geometric attempt to solve problems posed by a purely Greek theory of music (or to explain their insolubility in numbers).

Section III on "The Development of Systematic Deductive Mathematics" should interest the historian of philosophy as much as the historian of mathematics. For Szabó argues that the style and direction of Greek attempts to place mathematical knowledge on rigorous abstract logical foundations derived from the far-reaching influence of the Eleatics. They were directly responsible for the "anti-materialism" of Greek mathematical argument. They were indirectly responsible for particular axioms. The definition of number as a (finite) multitude of units represented, Szabó argues, a conscious rejection of Eleatic arguments against the possibility of multiplicity. The definitions of point, line, and plane purposely contradict Eleatic arguments against motion. These are only a few of the points raised; this third section contains in fact a plethora of information and analysis that defies brief resumé.

The book has its weak points, among them an annoying repetitiveness and a style frequently reminiscent of the scholastic *disputatio*. Szabó tends at times to rely on Proclus' testimony more heavily than he perhaps should from an historical point of view, overlooking, it seems, the distorting influences of neo-Platonism. By contrast, Aristotle receives relatively scant attention, as if the greatest logician and systematiser of Antiquity could have had little or no influence on Euclid and the systematisation of mathematics.

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NORDENSON, Harald (1969) *Relativity Time and Reality: A Critical Investigation of the Einstein Theory of Relativity from a Logical Point of View*. London: Allen and Unwin. £3.00 (60*s*.). Pp. 215.

There is a variety of ways in which contradictions can arise within science, but only a very small fraction of these contradictions compel us to reject a scientific theory categorically. A contradiction may be derivable from a formulation of a theory in conjunction with some philosophical position, from a theory in conjunction with some observed fact, or within the formulation of the theory itself. In the first two situations the theory can be saved by dropping or modifying the philosophical position, by rejecting or modifying the interpretation of the observed fact, or by reformulating the theory. Even when the third situation occurs it may be possible to save the theory by reformulating it, just as naive set theory was modified to avoid the inconsistencies arising from Russell's and other paradoxes. Only when there is no formulation free from contradiction that retains the central features of the theory can it be said to be necessary, for purely logical reasons, to reject the theory. From an historical point of view logical clarity and freedom from any kind of contradiction does not seem to be a common feature of scientific theories, past or present. Indeed, the genesis and growth of theories is more accurately portrayed by a very different description which can be characterised by a quotation from Nietzsche. "Unless a man has chaos in him he cannot give birth to a dancing star." However, all other things (such as scope, potential, aesthetic appeal) being equal, the more clearly a theory is formulated from a logical point of view the better.

It is by failing to discriminate between types of inconsistency that the author of the book under review is led to the drastic conclusion that Relativity Theory is logically incoherent, contains inconsistencies and must be rejected, even though he admits we have nothing to put in its place. He points out that an essential feature of Relativity Theory, both Special and General, is that in it the classical time concept is rejected and replaced by a new one. But he also seems to regard as essential the demand, made by Einstein in his early papers, that the new time concept be defined by means of experiments. It is from this extreme operationalist position that the trouble stems, for, in the specification of the experiments which purport to define relativistic time, classical time slips back into the theory. For example it is claimed that in the statement 'a light signal travels from A to B' the verb 'to travel' implies a time concept which the author takes to be the classical one, since we cannot allow the relativistic time concept that we are in the process of defining to appear in the description of the defining experiments. Once it is established that classical time as well as relativistic time is implied in Relativity Theory there is no difficult in showing that the very foundations of the theory crumble.

The author argues for the retention of classical time in favour of relativistic time on the grounds that there is no adequate acceptable definition of the latter. But he does not offer a definition of classical time that meets his own requirements. We are told that classical time is given *a priori.* It is not clear, however, in what sense this assertion is meant to be taken; whether '*a priori*' is to be interpreted as 'acquired through everyday experience' or whether it is to be taken in a Kantian or some other sense.

One of Dr Nordenson's arguments is based on an error of which others, including Keswani, have recently been guilty. Suppose a point P to move with velocity u with respect to some reference frame K, and a second point Q to move in the same direction with velocity w, also with respect to K. Then the velocity of P relative to Q, interpreted as the time rate of change of the distance PQ measured in K, is given by $u-w$. The fact that, for negative w, this velocity can be greater than the velocity of light is not inconsistent with the relativity postulates. No point moves with this velocity in K nor is any energy or information transmitted with this velocity. Dr. Nordenson condemns Einstein for combining velocities in this way in his early papers on Special Relativity. He claims that the relative velocity in question is correctly given by $\frac{u-w}{1+uw/c^2}$, a misapplication of the relativistic velocity transformation.

One third of the book is devoted to a review of the literature "of those authors who have most closely discussed the ultimate foundations of the theory". This section of the book tends to become monotonous. The discussion of the work of most of the authors is confined to a demonstration of the fact that they do not define the time concept to Dr Nordenson's satisfaction.

The author's main argument is certainly clearly, if repetitively, presented and it is quite a forceful one if it is taken to be an attack on an extreme operationalist philosophy of science or on some, or even all, of the existing formulations of the Theory of Relativity, in particular on Einstein's original formulation. But if the author's contention that his argument warrants the complete rejection of Relativity Theory is accepted then most scientific theories would have to be rejected on similar grounds.

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GRÜNBAUM, Adolf (1968) *Geometry and Chronometry in Philosophical Perspective.* Minneapolis: University of Minnesota Press. Paperback $3.45. Pp. 378.

This book has three chapters. The first is a republication of Grünbaum's well known essay 'Geometry, Chronometry and Empiricism' which originally appeared in *Minnesota Studies in the Philosophy of Science*, and much of the material of which appeared again in Grünbaum's 1963 work *Philosophical Problems of Space and Time.* The second, fairly short, chapter is an examination of the meaning and justification of the hypothesis that everything has doubled overnight, and consists mainly of a detailed rebuttal of views put forward in two recent articles by Schlesinger. The climax is the third chapter which constitutes about half the book. This is a very detailed examination and point-by-point rebuttal of Hilary Putnam's paper 'An examination of Grünbaum's Philosophy of Geometry' which appeared in volume II of *Philosophy of Science. The Delaware Seminar.* This chapter has a vigorous polemical time.

Grünbaum is not an easy writer for any beginner who wanted an introduction to the philosophical problems of space and time raised by physical theories. But once one has some acquaintance with the literature of the subject, Grünbaum is a writer always stimulating, often illuminating and quite often conclusively and importantly correct. There are passages again and again in this book where Grünbaum works out useful philosophico-physical points with thorough and clear mathematics. For example in the final section of chapter III, Grünbaum attacks Putnam's thesis that a world described by a system of physics P and a system of geometry E can always be redescribed so as to yield the same physical predictions in terms of an arbitrarily chosen metric g_{ik} and a new physics P′ without P′ including universal forces in Reichenbach's sense. This is shown to be false for the detailed example which Putnam considers of a physics P which postulates three kinds of forces—G (gravitational), E (electromagnetic) and I (interactional). Grünbaum shows that if (as Putnam allows) at some places E or G and so I vanish, P′ must necessarily include universal forces.

Grünbaum's detailed points are invaluable. What is less satisfactory is the overall viewpoint and the very general philosophical arguments used to back it up. Perhaps the fundamental thesis running through this work, as through other works of Grünbaum is his thesis, following Riemann and Poincaré, of "geochronometric conventionalism" (GC). Grünbaum admits the obvious thesis which he terms 'trivial semantical conventionalism' (TSC) that we are "free to decree the referents of the *unpreempted word* "congruent" because such freedom can be exercised with respect to *any* as yet semantically uncommitted term or string of symbols whatever" (p. 20). GC however is "the initially non-semantical claim that the continua of physical space and time each lack an intrinsic metre. And the metric amorphousness of these continua then serves to *explain* that even *after* the word "congruent" has been preempted semantically as a spatial or temporal *equality* predicate by the axioms of congruence, congruence remains ambiguous in the sense that these axioms still allow an infinitude of mutually exclusive congruence classes of intervals. Accordingly fundamentally *non*-semantical considerations are used to show that only a conventional choice of *one* of these congruence classes can provide a unique standard of length equality" (p. 21).

Now it is true that the rules for the use of 'congruent' can, perhaps somewhat artificially, be divided into two groups, formal rules which relate 'congruent' to terms like 'equal', 'point', 'line', 'interval', etc., and material rules ("correspondence rules") which state how to ascertain if two rods or temporal intervals are congruent. Among rules of the first group are such rules as that if two rods are congruent, then they have the same length, or that if two successive temporal intervals were congruent, then the total interval formed by the two was twice as long as the interval formed by each. Impressed by this difference between the two kinds of rules, Grünbaum has made two claims. The first is that the formal rules alone determine the intension of 'congruent' and that adoption of a set of material rules to determine its application is a matter of further convention. The second is that this is no trivial semantical truth, but shows something important about space and time, their "intrinsic metric amorphousness". The first claim seems doubtful, and the second is plainly wrong.

Let us begin by noting that, although Grünbaum does not bring this out, we can make just such a distinction between formal and material rules for their use with most other terms, including, to take one of Putnam's examples, colour terms. There are rules relating 'red' to other terms, such as the rule that if an object is red, it is coloured, and rules showing how to ascertain whether an object is red (e.g. the rule that an object is red if it is as like in colour to tomatoes, English pillar boxes, or strawberries as they are to each other). Now since Grünbaum takes it "to be a matter of objective visual fact" whether or not "a given color quale is the same as (or color-congruent to) the quale present in another color patch" (pp. 291 f.), he must hold that formal and material rules together determine the intension of colour terms, and this seems clearly right. For most other terms too, it seems correct that where there are rules of both kinds for the application of a term, they jointly determine its intension. And quite clearly we could have a sense of 'congruent' in which its intension was determined by material rules embodying our ordinary standards of congruence as well as by the formal rules to which Grünbaum alludes. I think that we do have such a sense, and that a surveyor, for example, uses the term 'congruent' with this intension. However the term 'congruent', like the terms 'point', 'plane', 'line', 'parallel', etc., was taken over by the pure geometer and used with its intension determined by the rules of his geometrical system, viz. the formal rules, alone. Now when the geometrical system comes to be reapplied to its original subject matter, we can say one of two things about the terms of the system. We can say that the intension of the terms is determined by the formal rules (these being the only rules, when the terms were used, unapplied, by the pure geometer) and that the new material rules are, unlike material rules for the use of most terms, not indicative of the intension of the terms. These material rules would merely embody non-intensional conventions for applying the term. Alternatively we can say that the new material rules make the terms more specific, give a more specific intension to the terms. Which we ought to say depends on our rules for the use of the term 'intension', and these do not seem clear enough for any judgment on the matter to be obviously the right one.

But even if the former way of speaking is correct—which is Grünbaum's first claim—it would only be correct because of the history of the term 'congruent'. We obviously *can* have, as I have pointed out, a sense of 'congruent' in which both formal and material rules determine the intension, and such a sense would not be a wrong one—how could it be? Hence, contrary to Grünbaum's second claim, GC is a semantical thesis and does not show the "intrinsic metric amorphousness" of Space or Time as some non-semantical truth.

All that I can understand by the 'intrinsic metric amorphousness' of Space is the fact that one measures distances not by counting parts of Space but by applying to Space rods and tapes (and a similar point can be made about Time). This seems a necessary truth. Grünbaum argues that it is a contigent truth which might in fact not hold of space. He claims that if Space were "granular", it would have an "intrinsic metric". The congruences and metrical attributes of "intervals" would be "intrinsic, being based on the cardinal *number* of space atoms" (pp. 153 f.). But one could only measure distances and establish congruences by calculating numbers of space atoms on the assumption that such atoms occupied the same volume as each other. But suppose there turned out to be a thousand space atoms between A and B and a million between B and C when, by our ordinary criteria, the distances AB and BC were congruent with each other, what would we say? To my mind, we would regard this as showing that some space atoms take up more room than others. The assumption could thus be shown to be empirically false. So to generalise, any "intrinsic" metric possessed by Space or Time would only be such because it satisfied "extrinsic" criteria. The fundamental metric, in other words, would remain the extrinsic one.

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BUTTS, Robert, Ed. (1968) *William Whewell's Theory of Scientific Method.* Pittsburgh, Penn.: University of Pittsburgh Press. $8.95. Pp. xi+358.

William Whewell was many things to Victorian intellectual life: a reforming Cambridge don, responsible—along with Herschel and Babbage—for introducing French analytic methods into the teaching of mathematics in English universities; a distinguished historian of science and Gothic architecture; a prime mover in the formation of the British Association; a poet and essayist; a moral philosopher and economist of some repute; and probably the most original philosopher of science England produced in the nineteenth century. A victim of his own many-sidedness, Whewell has been largely ignored in histories of philosophy. His eclectic and intricate philosophy, half-Kantian and half-Baconian, has made him an unwieldy entity for historians. Anglo-Saxon philosophers and historians of philosophy especially have found this scathing critic of the inductive *cum* empirical approach something of an embarrassing enigma. Until recently, they have (perhaps understandably) preferred the empiricism and utilitarianism of Mill to the idealism and moral systematics of Whewell.

During the last decade, however, there have been signs of a major renewal of interest in the philosophical writings of Whewell. Marcucci has done a monographic study of his idealism, Butts and Walsh have explored Whewell's theory of necessary ideas, Schneewind has examined Whewell's ethics, and there have been numerous investigations of Whewellian philosophy of science. Recently, Buchdahl and Laudan have undertaken a ten-volume edition of Whewell's *Works*.

But if Whewell has become a respectable subject of scholarly analysis, he has still remained inaccessible to students and general readers. All of his major works, whether in the original editions or in reprints, have been prohibitively expensive. This volume by Butts, therefore, should fill an important need, for it makes available in a single volume, and at a reasonable price, most of the essential methodological writings of Whewell. Containing excerpts from the *Novum Organon Renovatum* and the *Philosophy of Discovery*, Butts' book manages most successfully to blend some well known Whewelliana (*Of induction* and "On the Fundamental Antithesis") with several lesser known, but very important essays, including papers on the foundations of mechanics, the historiography of science, and Aristotelian and Newtonian theories of induction. I cannot fault Butts' criteria for selection, which manage to exhibit Whewell at his most perceptive, intertwining historical case studies with astute conceptual analysis.

The editor's introductory essay is clear and competent, although it occasionally tends to over-simplify Whewell's treatment of methodological problems. For instance, Butts argues that Whewell's important concept of the *consilience of inductions* "amounts to the now fairly well-established methodological principle . . . that a good hypothesis explains more than that which it was first introduced to explain" (p. 18). If that were all consilience amounts to, it would be a doctrine neither original with Whewell (a decade earlier, Herschel had argued that a test of a good hypothesis is whether we can extend "its application to cases not originally contemplated"), nor scarcely sufficient to serve as a potent analytical tool for assessing the merits of scientific theories. On my reading of the texts, Whewellian consilience is rather richer than Butts allows, containing measures not only of content, but also of *novelty* and *systematicity*. Contrary to Butts' suggestion, it is possible to have a consilience of inductions which is *not* content-increasing; if we are able to reduce two or more *known* laws to a single law or theory, such a formal systemic gain is a consilience. Alternatively, a consilience is achieved when an hypothesis explains a *surprising* fact, even if that fact were the one which the hypothesis was introduced to explain. In sum, a consilience of inductions occurs whenever (a) *formal* simplification or unification is achieved, (b) an hypothesis predicts *new* facts, and (c) the hypothesis explains *known* facts not satisfactorily explained hitherto. If, like Butts, we will draw parallels with present-day doctrines, Whewellian consilience embodies the demand for theoretical simplicity and the Popperian notion of severe tests, as well as the more mundane requirement that an hypothesis should go beyond its evidence.

In spite of such interpretative reservations about parts of the introduction, this volume is most welcome. By putting Whewell within the reach of students and non-devotees, it may do more to promote the general study of Whewell's methodology than volumes of scholarly exegesis.

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The Scientific Intelligibility of Absolute Space:

A Study of Newtonian Argument

by HUGH M. LACEY

1 THERE IS a long tradition of discussion concerning the origin and logical status of Newton's Laws of Motion. Although necessarily this has involved some consideration of absolute space, it has not involved enough. In recent years it has been widely assumed, on grounds associated with operationalism and the verifiability theory of meaning, that Newton's employment of absolute space in the *Principia* is ill-founded. It is assumed that absolute space cannot be taken seriously as a component of mechanical explanations and that the *Principia* can be readily reconstructed without it. Newton's own employment of the notion is dismissed as due to his imposing his eccentric metaphysical beliefs upon the conceptual foundations of his science. Absolute space is a metaphysical appendage, unworthy of the serious attention of the empirically oriented scientist.

The neglect of absolute space is doubly unfortunate: it obscures the actual role that absolute space played in Newton's mechanics, and it obscures the realisation that those who reject Newton's arguments on this matter do so on grounds which involve a different conception of scientific laws from that which Newton held. A consequence of this neglect is that philosophers of science have devoted much time to the reconstruction of Newtonian mechanics, but little to Newton's own arguments. Yet these arguments are deep, as I propose to show. It is a good methodological principle of the philosophy of science not to reject too readily the commitments of a scientist of the stature of Newton. The scientific practice of a Newton is likely to reveal more about the nature of science than *a priori* epistemological and semantical doctrines. I hope to show that Newton's employment of absolute space is theoretically and methodologically proper, even if today there are grounds for holding that absolute space does not exist, or at least that there are no grounds for believing that it does exist.

Things are often spoken of as 'in space', where there appears to be a distinction between things and space. The central point at issue in this essay is: 'Is space, conceived of as distinct from things, among our ontological commitments?' Or: 'Is space to be included among the entities which we

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regard as existing, in addition to the things which we speak of as 'in space'?' Newton's answer is clear:

> Absolute space, in its own nature, without relation to anything external, remains always similar and immovable. Relative space is some movable dimension or measure of the absolute spaces; which our senses determine by its position to bodies, and which is commonly taken for immovable space; such is the dimension of a subterraneous, an aerial or celestial space, determined by its position in respect of the earth. Absolute and relative space are the same in figure and magnitude; but they do not remain always numerically the same. (Newton (1962), p. 6.)

For Newton, absolute space is real, and distinct from observable bodies. Equally as clearly, Leibniz states an opposing position:

> . . . I hold space to be something merely relative . . I hold it to be an order of coexistences, as time is an order of successions. For space denotes in terms of possibility, an order of things which exist at the same time, considered as existing together; without enquiring into their manner of existing. And when many things are seen together, one perceives that order of things among themselves. (Leibniz, third letter to Clarke, Alexander (1956), pp. 25–26. Compare with the more thorough account in Leibniz (1951), pp. 201 ff.)

My procedure in this essay will be the following: first to clarify a number of distinct theses associated with absolute space, secondly to survey briefly some of the *a priori* arguments for and against absolute space, and finally to examine the logic of Newton's argument for it, based upon the bucket-experiment. I will show how Newton's contemporaries failed to meet his argument, and I will explore what characteristics a relational theory must have to meet this argument.

2 THESES ABOUT ABSOLUTE SPACE

It is common to find absolute space contrasted with both relational space and relative space. These are different contrasts based upon different theses about space. That this is not always (or even often) recognised may be due to the fact that Newton and Leibniz used the word 'relative' where modern writers tend to use 'relational'.

2.1 *Absolute–Relational*

The debates between Newton, Leibniz and their followers pertain to this distinction. The central elements of the two theories may be summed up as follows, where 'A' indicates theses of the absolute theory and 'R' corresponding theses of the relational theory.[1]

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[1] Similar characterisations occur in Broad (1923), and Russell (1903). The characteristics of and contrasts between the two theories of space are most illuminatingly presented by means of complete formalisations of the theories. Such formalisations will not be developed in this essay, however, as the amount of technical detail involved would obscure

(*a*) A. There are two kinds of irreducible spatial entities (that is, entities among which spatial relations hold): (1) pure spatial entities—points, positions or places; (2) bodies or physical objects which occupy points or places.

R. There is only one kind of irreducible spatial entity—bodies.[1]

(*b*) A. Pure spatial entities are ontologically independent of bodies, that is, space can exist in the absence of all matter. But bodies cannot exist without space; a body must occupy a place. (Compare with Wittgenstein: 'We cannot think of spatial objects at all apart from space' (1922) 2.0121.)

R. If there are no bodies there is no space. Space is the totality of bodies ordered by the spatial relations.

(*c*) A. The set of points and thence space is continuous and infinitely extended.

R. Whether or not space is continuous and infinitely extended is open to empirical investigation; it is not a conceptual matter.

(*d*) A. Spatial relations among points are invariant with time, those among bodies are variable. Space neither operates upon nor is operated upon by bodies.

R. All spatial relations are variable (at least in principle). All objects can enter into causal relations.

(*e*) A. The spatial relations and properties of bodies are ontologically (though not epistemologically) derivative from the relations and properties of the places which they occupy.[2]

R. Some spatial relations among bodies are primitive.

(*f*) A. Points and the spatial relations among them are not observable.

R. Some spatial relations among some bodies are observable.

(*g*) A. The set of points exhibits a metrical structure. Such a structure is not imposed upon the set by bodies, although it is from the observable

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the main point of the essay. It is worth mentioning that in such a formalisation the purely spatial entities satisfy Hilbert's postulates (Hilbert, 1930).

[1] In contemporary discussions the class of spatial entities which are not pure spatial entities is larger than the class of bodies. It must also include fields and, probably, events. In the absolute theory all such additional entities would also be said to occupy points or places.

[2] Formalisation makes this characteristic transparent. A useful formalisation is within a many-sorted logic with two sorts of individuals and individual variables.
Let x^1, y^1, z^1 be points or places.
Let x^2, y^2, z^2 be bodies.
Let O be the relation of 'occupation' which bodies bear to places, so that '$O(x^2,x^1)$' reads 'x^2 occupies x^1'.
Let R be a spatial relation (e.g., 'betweenness') which is a primitive among places.
Then, the ontologically derivative character of R among bodies is expressed by the definition:

$$R(x^2, y^3) = \text{df.}\ (\exists x^1)(\exists y^1)[O(x^2, x^1).\ O(y^2, y^1).\ R(x^1, y^1)]$$

relations of bodies that we discover which metrical relations hold among given points. (For Newton the metric of space is Euclidean.) In other words, not only do relations like 'betweenness', 'lies on the same line as' exist among the points, but also relations like 'is the same distance from', and such relations exist independently of the bodies which occupy the points.

R. Independently of the relationships of bodies to a special class of bodies (measuring rods) there are no metrical relations. Moreover, once given a characterisation of "measuring rod" (or other measuring procedure) it is only empirical investigation which reveals the metric of space.

(*h*) A. There are two kinds of ***motion***. A body moves ***absolutely*** if and only if it occupies different places at different times. A body moves ***relatively*** if and only if there is another body some of whose spatial relations with the former body vary with time.

R. There are only relative motions.

In his discussion of the bucket-experiment, Newton argued for the existence of absolute motion and thence for the existence of absolute space, where by 'absolute space' is understood the set of pure spatial entities.

2.2 *Absolute–Relative*

Consider any spatial relation, R, holding among a class of spatial entities (of any kind) $S_1, \ldots S_n$. For example, R might be 'between' holding among the ordered class of bodies, A, B and C, or R might be 'separated by a distance of 8 feet' holding on A and B. The relative theory maintains that the expression '$R(S_1, \ldots S_n)$' is essentially incomplete, being completed only by the specification of a further object (frame of reference) with respect to which the designation is made. Furthermore, in general if '$R(S_1, \ldots S_n)$ with respect to O_1' is true, then '$R(S_1, \ldots S_n)$ with respect to O_2' is not true. It is in this sense that spatial terms such as length are held not to be absolute but relative to a frame of reference. On the other hand, the absolute theory (understood as that theory which opposes the relative theory) maintains either that '$R(S_1, \ldots S_n)$' is a complete expression, or that the designation is invariant with frame of reference. Obviously such a relative theory can be held with various degrees of comprehensiveness. For example, it could be held in the case of the metrical properties of space but not the topological ones.[1] Also, while it is clear that the absolute theory of 2.1 is not compatible with the relative theory (unless it is expanded so that an absolute space is postulated to correspond to each physical frame of reference), on the other hand the relational theory is compatible with either of the theories of 2.2. Both Newton and Leibniz, for example, held that spatial relations at a given time are absolute in sense 2.2, but they disagreed on the theses contrasted under 2.1.

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[1] Similar contrasting theories, "absolute–relational", "absolute–relative" exist in the case of time. The special theory of relativity upholds the relational theory of time, but the absolute (opposite of relative) for *some* topological relations, viz. the topological relations between those events which are causally connectible, though not those between other spatially separated events.

For the remainder of this essay I will use the term 'absolute space' only in the sense of 2.1. The contrast is 'relational space', not 'relative space'.

3 A PRIORI ARGUMENTS CONCERNING ABSOLUTE SPACE

In this section I will summarise briefly the major *a priori* arguments for and against the absolute theory and I will show why none of them is decisive.

3.1 *Arguments Against Absolute Space*

(*a*) What of its nature cannot be observed does not exist. Thence neither absolute space nor absolute motion exists. Consider Leibniz:

> I answer, motion does not indeed depend upon being observed; but it does depend upon being possible to be observed. There is no motion, when there is no change that can be observed. And when there is no change that can be observed, there is no change at all. (Fifth letter to Clarke, Alexander (1956), p. 74.)

A weaker version of this argument might hold that there can be no evidence for the existence of that which cannot be observed, so best keep such out of science.

Neither version is decisive, for each fails to take into account the possibility that the unobservable in principle may have observable effects, thus leaving it open that a theoretical argument might confirm the existence of the unobservables.

(*b*) Absolute space neither acts nor is acted upon, and so is not real. Since the points of space are identical in all properties, it is indifferent to the outcome of an action whether it happens at one point or another. Equally, every action leaves space unchanged. Leibniz exploited this absence of causal properties when he argued (third letter to Clarke, Alexander (1956), p. 26) that God could have no reason to place an object at *A* rather than at *B*, and thence that *A* is not distinct from *B*. The point of the argument is that absolute space violates a proposed criterion of what is real: having causal properties.

The causal argument overlooks the possibility that absolute motion may have effects, so that it may be real according to the criterion. And then logical analysis reveals that a necessary condition of its reality is the reality of absolute space. Leibniz's argument overlooks the possibility that objects which are identical in their properties may be individuated by their relations among each other.

(*c*) Grünbaum (1963, ch. 1) has argued that a continuous set of qualitatively identical dimensionless points cannot possess an intrinsic metric, because any two intervals of the set have identical cardinality, and the set contains no other properties which might be used to distinguish the lengths of different intervals. (It is ironical to note that Leibniz was unable to refute Clarke: that relational theories, unlike absolute ones, cannot give an account of the metrical properties of space. Alexander (1956), p. 75.)

I find this argument unconvincing. Grünbaum allows that such points may exhibit primitive topological relations among each other. I see no reason why they should not also exhibit primitive metrical relations among each other, and no reasons why metrical relations *must* be grounded upon other properties. I see no reason why the logical status of 'is between . . . and . . .' and 'is the same distance from . . . as . . .' must be different. Of course, if *we* are to distinguish different intervals in length, we must do so in virtue of some other property of the interval (or some relation of the interval to another body), but there may be distinctions in reality, which we assert for theoretical reasons, but which we cannot directly determine. But even if Grünbaum's argument is valid it counts only against clause (*g*) of the absolute theory.

(*d*) Whenever we say that a body *A* moves, our judgment is based upon *A*'s relative motion with respect to some other body *B*. This suggests that the proper logical analysis of '*A* moves' is 'There is a body *B* such that *A* moves with respect to *B*'. On such an analysis absolute motion would make no sense and so it could not have effects, *via* which it could have a theoretical role. (This argument is suggested by a passage in Mach (1960), p. 342.)

Such an analysis, however, cannot be decisive, for in the interests of theory there is nothing to prevent the introduction of a more general concept of motion, which does not restrict the domain of *B* to bodies, but extends it to all spatial entities (observed or inferred). Theory is not confined to phenomenology. Mach (1960, p. 341) attempts to cut short this counter-argument by maintaining that entities not given directly in experience are pure mental constructs and thence, by implication, do not refer to reality. The invalidity of this counter-move should be transparent.

(*e*) 'Absolute space' does not accord with the requirements of the verifiability theory of meaning. Certainly it does not accord with operationalist requirements. But in the light of the argument in section 4, it is very doubtful that it does not accord with the more liberal versions of the verifiability theory.

3.2 *Arguments for Absolute Space*

(*a*) Absolute space is presupposed in the statements of the laws of motion and the law of gravitation, and there is no adequate reconstruction of these laws without the presupposition. This issue is too complex to be discussed in this essay.

(*b*) Space is distinct from physical objects, for if a physical object is annihilated its extension will remain. Suppose that there is a container full of water and that the water is annihilated, and that no other substance enters into the container to replace the water. Then we do say that there is an empty space present, suggesting ontological commitment to space. But such commitment is not entailed, for equally as well we could say that the walls of the container are a finite distance apart and that there is no object between them.

(*c*) There are serious difficulties in adequately formulating relational theories. The general intent of relational theories is easy to express: that space is a structure of ordering relations and that space-talk involves commitment to no objects other than the objects of which we speak as 'in space'. Such a statement, however, is no more than the outline of a programme that needs to be carried out in detail. Usually theories of space are constructed upon a set-theoretic foundation, that is, space is taken to be a set of objects ordered by the usual spatial relations. According to the intent of relational theories the set of physical objects should be sufficient for the construction of space. This is a severe limitation for it prevents even the formulation that space is continuous (at least, unless we also call upon fields, but then the formulation of the continuity of space becomes dependent upon the details of advanced physical theories). It is a necessary condition for being a continuous space that between any two objects of the space there exists another. Taking any two non-contacting physical objects it is not guaranteed that there is another between them. The standard solution is to fill up the interval between them with points; more generally, to fill up all "empty space" with points. Within a set-theoretic foundation it is hard to see how continuity can be preserved in any other way (so long as we are referring only to physical objects). In other words, some pure spatial entities seem to be necessary in order to maintain a fundamental property of space. This argument is not conclusive as I will show in another paper. Nevertheless, empirical considerations aside, before the relational theory can be vindicated its foundations must be clarified.

4 THE BUCKET-EXPERIMENT[1]

It is not generally realised that Newton's discussion of the bucket-experiment (1962, pp. 10–12) contains a theoretical argument for the existence of absolute space. It is usually taken for granted that his discussion is no more than a token attempt to rationalise his metaphysical beliefs. It is true that Newton had metaphysical reasons for asserting the existence of absolute space and absolute time, since he equated absolute space with God's infinity and absolute time with God's eternity (1962, p. 545). Newton's disciple, Clarke, was most concerned to defend these metaphysical positions (Alexander, 1956). It served these metaphysical conceptions nicely to bolster them with scientific arguments. I will show, however, that Newton's discussion is not rationalisation, but that it constitutes such a powerful argument that his contemporaries, Leibniz and Berkeley, were unable to construct relational theories which answered it. If I am right there were genuine (though with hindsight we can say, not decisive) physical reasons behind the central place of absolute space in the basic theory which guided physics for two centuries. In placing it in this central place, physicists were led by genuine physical intuition, not misled, as some have suggested, by the authority of Newton.

Unfortunately Newton did not present his argument very clearly and he omitted to state some premises which he shared with his opponents. I will attempt to reconstruct the argument fully.

4.1 Newton was well aware that on the level of observation only relative motions can be discerned, and that in kinematics no case can be constructed for the existence of absolute space, for (except from the point of view of descriptive simplicity) it is indifferent which frame of reference we choose as the reference base for the description of motions. The Copernican and Ptolemaic planetary systems are kinematically equivalent. But the matter does not rest there. When discussion includes dynamics, that is, the domain in which we not only describe motions but also explain them or consider their causes, it is possible, Newton maintains, to distinguish absolute from relative motions. (The Copernican and Ptolemaic systems are not dynamically equivalent.)

> The causes by which true and relative motions are distinguished one from the other are the forces impressed upon bodies to generate motion. True motion is neither generated nor altered, but by some force impressed upon the body moved; but relative motion may be generated or altered without any force impressed upon the body.... Again, true motion suffers always some change from any force impressed upon the moving body; but relative motion does not necessarily undergo any change by such forces. (Newton (1962), p. 10.)

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[1] An outline of the analysis presented in this section is contained in an appendix to my paper, Lacey (1969).

Absolute motions may be detected indirectly by an effect of absolute motion, viz. the forces of receding from the axis of circular motion. In contemporary jargon, 'absolute motion' is a theoretical term; it functions in theoretical explanations of certain observed phenomena. Typical of such phenomena are those observed in the course of the bucket-experiment.

Consider an ordinary bucket part filled with water and suspended by a long cord. The experiment proceeds in two stages.

1. The cord is strongly twisted and the bucket is held at rest. The water and the bucket are relatively at rest. The surface of the water is observed to be horizontal.

2. The bucket is released and set rotating by a strong force. At first no change is observed in the surface of the water. After a time the surface becomes concave (as the bucket transfers its motion to the water and the water attempts to recede from the axis of motion). The depth of the concavity is dependent upon the speed of rotation. Finally a state of equilibrium is reached in which the bucket and the water are relatively at rest, and the surface still concave.

Newton's discussion of the experiment proceeds according to the following steps:

(*a*) At equilibrium in both of stages 1 and 2, the bucket and the water are relatively at rest, yet in one case the surface is horizontal, in the other concave. (This is an observational report.)

(*a′*) At the beginning of stage 2, when the relative motion is greatest, the surface remains horizontal. (Another observational report.)

Therefore:

(*b*) The shape of the surface of the water is not dependent at any moment of the experiment upon the motion of the water relative to the bucket.[1]

Therefore, Newton concludes:

(*c*) The concavity is a consequence of the water undergoing absolute motion.

The absolute motion is, of course, an absolute acceleration, not an absolute velocity. So the experiment (even assuming that it has been correctly interpreted) does not enable us to pick out the actual absolute frame of reference, or a physical frame at rest with respect to absolute space. Nevertheless, if Newton's interpretation is correct, and his analysis of absolute motion is valid, then it does show that it is theoretically necessary to affirm the existence of absolute space.

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[1] This conclusion may be seen even more clearly if we add a third stage to the experiment: Suddenly the bucket is stopped. At first the concavity remains. Then gradually the original state of the water is regained. (*b*) also follows in the same way if in place of (*a*) and (*a′*) we use the observational report: at the beginnings of stages 2 and 3 the relative motion of the water with respect to the bucket is greatest, yet in the one case the surface remains horizontal, in the other concave. The change in the relative motion of the water and the bucket induces no change in the surface.

Newton's interpretation, as presented above, is unconvincing, since the inference from (*b*) to (*c*) is plainly invalid. Does Newton's argument fail because of an elementary logical mistake?

In an early writing (1965a), Newton produces an argument which fills this logical gap—rather an argument designed to show that Cartesian theory does not show the inference to be unjustified. Only a hint of this argument remains in the *Principia*:

> There is only *one real* circular motion of any one revolving body corresponding to only *one* power of endeavouring to recede from its axis of motion, as its proper and adequate effect, but relative motions, in one and the same body, are *innumerable*, according to the various relations it bears to external bodies, and, like other relations, are *altogether destitute of any real effect*, any otherwise than they may partake of that *one only true* motion. (Newton (1962), p. 11. My italics.)

4.2 In (1965a) Newton agrees with Descartes that a body has only one proper motion, where by 'proper motion' is meant 'a motion which produces effects'. For Descartes the one proper motion is a relative motion, the motion of a body from the neighbourhood of one set of bodies to the neighbourhood of another set. Motion is, in the words of Descartes:

> The transference of one part of matter or one body from the vicinity of those bodies that are in immediate contact with it, and which we regard as in repose, into the vicinity of others. (*Principles of Philosophy*, Bk. 2, ch. XXV (1955), p. 266.)

Newton argues that this doctrine has absurd consequences:

> Now truely not only the absurd consequences of this doctrine prove how confused and foreign to reason it is, but Descartes himself by contradicting himself seems to acknowledge this to be the case. For he says the Earth and other planets are at rest speaking truely and in the philosophical sense, and that he who says it is moved on account of its translation relative to the fixed stars lacks reason and speaks only in the vulgar sense. But later he nevertheless posits in the Earth and the planets an endeavour to recede from the Sun as if from a centre about which they are moved, by means of which and the like endeavour of the revolving vortices they are poised at their distances from the Sun. What then? Is this *conatus* to be derived from the Planet's rest, according to Descartes true and philosophical, or from vulgar and non-philosophical motions? (Newton (1965a), p. 227. Compare with the translation in Hall (1962), p. 124.)

The force of the argument is clear: how can a property proper to a moving body be present in a body truly at rest? Yet as presented, the argument is not quite decisive. For Descartes might avail himself of his dual theory of motion, according to which a body has but one true motion, although it participates in the motions of neighbouring bodies fixed with respect to it.

Then he might explain the endeavour of the earth to recede from the sun as due to its participation in the motion of the vortex containing it. No such reply, however, can be made to the evidence of the bucket-experiment. In it, there are effects to be attributed to a motion, but which cannot be attributed to the motion of the water with respect to its neighbouring bodies (the bucket), nor can the effect be explained in terms of the neighbouring bodies participating in a rotary motion. Thus no relational theory of the Cartesian type can account for the phenomena of the bucket-experiment.

The refutation of the Cartesian theory of relative motion does not close the logical gap between (*b*) and (*c*), since the positions of Newton and Descartes do not exhaust the logical possibilities. Newton (1965a) appears to be aware of this. Having argued above that if the proper motion of a body is a relative one, then there are no grounds upon which we can select Descartes' motion from among all the other relative motions as the true and proper one, he appears to accept that no other method is available for picking out a relative motion as the proper motion. Consequently all relative motions are equally proper. But each moving body has a multiplicity of relative motions, and thence has effects due to each of them, which is absurd. So the proper motion is not a relative one. Newton's exact words are:

> . . . it appears moreover that of motions (relative) no one can be said to be true, absolute, and more proper beyond the others, but that all in respect of both neighbouring and remote bodies are equally philosophical, than which we are able to imagine nothing more absurd. For unless a unique physical motion is allowed to any particular body, additional changes of its situation and position among other bodies being regarded as external denominations only, it will follow, for example, that the Earth will have an endeavour to recede from the centre of the Sun on account of its motion relative to the fixed stars, and a less endeavour on account of its less motion relative to Saturn and the aetherial orbit in which it is carried, and so much the less again relative to Jupiter and the surrounding aether composing its orbit, and much less relative to other aetherial orbits which carry no planets and are closer to the annual orbit of the Earth, and indeed relative to its own orbit no endeavour to recede at all since it is not moved in that orbit. And since all these endeavours are incapable of absolute agreement it must rather be affirmed that there is only one natural and absolute motion appropriate to the Earth thanks to which it endeavours to recede from the Sun, and that its translations relative to external bodies are external denominations only. (Newton (1965a), pp. 227–8. Cf. Hall (1962), p. 127.)

In the passage from the *Principia*, quoted at the end of 4.1, the conclusion of this argument remains, but not the argument itself. In the *Principia* he is content to rely on the general doctrine that no relations have real effects of their own. The bucket-experiment illustrates a particular case of the general doctrine. But its absence from the *Principia* does not render this argument irrelevant to the evaluation of Newton's thought, for the argument provides premises which suffice to close the logical gap between (*b*) and (*c*), for it contains or presupposes the following assertions:

(*d′*) A body (at a given time) has only one proper motion.

(*e′*) The motion of a body A relative to B (at a given time) is a different motion from that of A relative to C (at the same time). (For example, the motion of the earth relative to the stars is a different motion from that of the earth relative to the sun.)

(*f′*) If one relative motion involving A (e.g., the earth) has effects, so must all relative motions involving A, (unless the relative motion is that of A relative to its neighbouring bodies, but the previous argument rules out this relative motion having effects).

(*g′*) These effects are not in agreement, and (presumably) the combination of them would not agree with the observed effects of a motion (e.g. that of the earth.)[1]

From (*d′*), (*e′*) and (*f′*) can be inferred:

(*h′*) The proper motion of a body is not a relative one (since the proper motion of a body is that which produces the effects of its motion).

Strictly, (*g′*) is irrelevant to the argument. But (*h′*) can also be inferred from (*e′*), (*f′*) and (*g′*), and indeed in the quoted passage, the role of (*g′*) appears to be that of additional backing for (*d′*), via a ***reductio ad absurdum*** argument in which the falsity of (*d′*) in entertained. Since the proper motion of a body is either relative or absolute, (*c*) follows from (*h′*).

(*d′*) is an assumption and (*e′*) an analysis which Newton shares with Descartes. The unsoundness of the argument springs from their combination. There is nothing improper in an analysis which individuates motions differently, and according to which the earth, at a given time, has only one motion which is relative to the fixed stars, and to the sun, and to . . . , and an hypothesis that the effects due to motion are due to the motion being with respect to a framework somehow determined by all the objects of the universe. More explicitly, we may define 'A moves at t' as 'There is a body B, such that A moves with respect to B at t'. Suppose that X is the class of non-neighbouring bodies with respect to which A moves at t. Then we may hypothesize that the effects of A's motion are a function of all of the relative motions of A with respect to members of X. It is consistent with this hypothesis that the effects be a function of what other objects there are and their distribution in the universe. Then we may say that the effects are due to the motion of A with respect to a set of axes determined by the objects of the universe and their distribution—a set of axes which remains more or less constant since the class of objects of the universe and their distribution are approximately constant. (How the set of axes is thereby determined need not detain us.) The motion of *A* with respect to this set of axes would then be its proper motion, if any longer this notion be considered useful.

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[1] The second conjunct of (*g′*) does not occur explicitly in Newton, but it is necessary in order to maintain the validity of the argument to (*h′*) from (*f′*) and (*g′*), and in order that (*g′*) serve as part of the backing for (*d′*).

If (e') is replaced by the new analysis of motion, the conclusion no longer follows. Alternatively, one could challenge (f') (which Newton simply asserted), and maintain that there is a privileged set of objects such that motion with respect to it and no other produces effects. (Other challenges to the argument are also possible.) Newton does not seem to have perceived the serious possibility that it might be a relative motion of *A* with respect to bodies other than its neighbouring bodies which produces the effects of *A*'s motion. The combination of (d') and (e') dimmed the possibility of such perception. Thus Newton's argument (1965a) is decisive against Descartes' theory of relative motion, but it leaves open the possibility of more sophisticated theories of relative motion.

Another argument appears in an early draft of the *Principia* (1965b). Newton says:

> But absolute motion is in fact distinguished from relative in circular motions by the endeavour to recede from the centre, which . . . in a circular motion relative to bodies at rest may be very large, as in the celestial bodies which the Cartesians believe to be at rest, although they endeavour to recede from the sun. The fact that this endeavour [from the centre of circular motion] is certain and definite argues some certain and definite quantity of real motions in individual bodies in no wise dependent on the relations [between the bodies] which are innumerable and make up as many relative motions. For example, that motion and rest absolutely speaking do not depend on the situation and relation of bodies between themselves is evident from the fact that these are never changed except by force impressed on the body moved or at rest, and are always changed after [the action of] such a force (Newton (1965b), p. 310.)

The argument (to the extent that it differs from the previous one) appears to be: the circular motion of a body *A*, as evidenced by its centrifugal effects, changes only and always when a force is impressed upon *A*, and never when a force is impressed upon other bodies. So the motion, producing these effects, is a motion over and above the relative motions of *A*. Now, the first statement can be interpreted either as a dogmatic assertion, in which case there is no strength to the argument, or as an observational report. On the latter interpretation, the argument fails, because it overlooks the fact that the motions which we actually observe are all motions relative to the fixed stars, and that we can only observe the effects of the changes of some bodies. That the motion of some observed bodies does not observably effect the motion of *A* does not entail that the motion of all bodies or of some other bodies will not have such an effect.

Thus I conclude that the Newtonian arguments given so far do not adequately fill the logical gap between (*b*) and (*c*), ***provided that*** one's relational theory does not individuate motions in the Cartesian manner, and thus allows that centrifugal effects are due to the relative motion of a body with respect to an axis determined by all the bodies of the universe, or that one's relational theory allows that there is a special set of objects such that motion relative to it causes centrifugal effects. Newton (1965a and b) did not entertain the possibility of relational theories with such characteristics.

4.3 The matter does not rest here, however, for in the *Principia* Newton goes on to consider a "thought-experiment". He does not make clear what the point of the thought-experiment is. Explicitly it is presented as an illustration of how absolute motions can be determined. At the same time it is clearly an argument for the *possibility* of absolute motions (even in the absence of relative motions), and I think also for the real existence of absolute motions.

Consider two globes connected by a cord which keeps them a constant distance apart. Suppose that they are situated in an otherwise empty universe so that no observable comparisons can be made between the globes and other bodies. Because the cord keeps the globes a constant distance apart there is no relative motion between them. Nevertheless, Newton maintains that the globes could be in motion and that if they are in circular motion about their centre of gravity, this motion can be detected and measured by the amount of tension in the cord. Suppose now that we consider the globes in a universe together with the fixed stars, very distant from them. Again the globes are a fixed distance apart. They are rotating relative to the fixed stars. If there is a tension in the cord it is the globes that are truly rotating. If, on the other hand, the relative motion is observed but not the tension in the cord, then it is the stars and not the globes which are truly rotating.

Given the context of the presentation of this thought-experiment, viz. that it occurs immediately after discussion of the bucket-experiment, it is surely no accident that it highlights a crucial point at issue with upholders of relational theories. In the first part of the thought-experiment, since there is no relative motion, according to the relationalist there is no motion at all—and without a motion, there *cannot* be a tension in the cord (unless Newton's laws of motion, as well as his interpretation of them, are wrong), and there cannot be any variation of tension. For the absolutist the existence of tension would be a symptom of motion; for the relationalist there cannot be a tension in the cord. If we could produce the two-globe universe, we would have a potential empirical test between the two theories. In the second part of the thought-experiment, if there is a tension in the cord, it is due to a centrifugal force generated by a motion; for the relationalist, by a relative motion between the globes and the fixed stars. For him, therefore, there can only be one outcome of such a relative motion, not two depending upon which of the globes or the stars is truly at rest and which is truly in motion. No matter whether the relative motion is initiated by a force acting on the stars or a force acting on the globes, for the relationalist the same tension should eventuate. If this is accepted, it follows that a force acting on the fixed stars can cause a tension in the cord. Therefore, causal action at a distance is possible.

Another way in which to see that the relationalist is committed to causal action at a distance is to reverse the order of the parts of Newton's thought-experiment. Consider the globes rotating relatively to the fixed stars, so that there is a tension in the cord. Then, suppose the fixed stars annihilated, would the tension remain in the cord? According to Newton, yes: according to the relationalists, no: since motion—and thence the tension due to motion—would no longer be possible. Once again in the latter case, a causal action on the stars has repercussions on the cord.

This analysis of the thought-experiment of the globes provides, I think, a key to filling in the steps between (*b*) and (*c*). The analysis illustrates the truth of:

(*d*) If centrifugal effects are dependent upon the relative motion of objects not in contact with each other, then there is causal action at a distance.

Newton's conclusions regarding the thought-experiment rest upon:

(*e*) There is no causal action at a distance.

(*f*) If the shape of the surface of the water is dependent upon a relative motion between the water and an object (or set of objects) not in contact with the water, then there is causal action at a distance. (This is a particular case of (*d*).)

(*g*) The shape of the surface of the water is dependent upon some motion, either an absolute motion, or a relative motion between the water and some object(s). (Newtonian theory requires this.)

Since the bucket is the only object in contact with the water, (*b*), (*e*) and (*f*) suffice to give:

(*h*) The shape of the surface of the water is not dependent upon any relative motion.

Then (*g*) and (*h*) yield (*c*). Thus the above statements suffice to close the logical gap between (*b*) and (*c*). Furthermore all the statements are acceptable to Newton. However, whether or not the argument so filled out captures Newton's intent, it provides a useful analysis against which to consider attempts to dismiss the bucket-experiment as evidence for the existence of absolute space—as well as being an important argument in its own right.

4.4 None of the statements (*d*), (*e*) and (*g*) is beyond dispute.

It might be claimed that Newton himself did not accept (*e*), on the basis of his acceptance of the law of gravitation, which, on the face of it, appears to involve action at a distance. But I think that there is no doubt that Newton saw no incompatibility between acceptance of the law of gravitation and acceptance of (*e*). Indeed in his famous third letter to Bentley, he expressly regards as absurd any interpretation of the law of gravitation which involves action at a distance.[1] I will not discuss Newton's views on (*e*) further in this essay.[2] Mach, of course, rejected (*e*).

4.5 Leibniz rejected (*g*). According to him, the water's surface is affected only when the immediate cause of the relative motion between the water and the fixed stars (for any motion which *we* observe in the water of the bucket is—whatever else it be—also a motion relative to the stars) is in the water. He says:

> For when the immediate cause of the change is in the body, that body is truely in motion; and then the situation of other bodies, with respect to it, will be changed consequently, though the cause of the change be not in them. (Leibniz, fifth letter to Clarke, Alexander (1956), p. 74. Cf. p. 70.)

The immediate cause is a force which is real. In his *Dynamics*, he says:

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[1] Newton writes:

> It is inconceivable that inanimate brute Matter should, without the mediation of something else, which is not material, operate upon, and effect other matter without mutual contact, as it must, if gravitation in the sense of Epicurus be essential and inherent in it. And this is one reason why I desired you would not ascribe innate gravity to me. That gravitation should be innate, inherent and essential to matter, so that one body may act upon another at a distance, through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers. (Newton, in Cohen (1958), pp. 302–3. Cf. the preface to 1717 edition of the *Opticks*.)

[2] It is interesting to note that the argument could still be carried through if (*e*) were replaced by the weaker statement: 'There is no causal action at a distance in the circumstances under consideration.' This weaker statement might be defended by maintaining that there is a well-confirmed theory (viz. Newton's with the laws of motion framed with respect to absolute space) in which the phenomena of the bucket-experiment are explained without commitment to causal action at a distance. (That Newton's explanation is indeed *an* explanation is vindicated by the above analysis.) And the alternative theory, in which action at a distance is employed, has not (at least in Newton's time) been adequately stated, much less confirmed. This sort of confirmation of the weaker statement should be acceptable to anyone who does not maintain that proper scientific method involves the multiplication of theories.

'... we must recognise that force is something absolutely real even in created substances but that space, time and motion are of the nature of relations.' (Cf. Alexander (1956), p. xxvi.) Leibniz presumably accepted (*d*).[1] He certainly accepted (*e*):

> A body is never moved naturally, except by another body which touches and pushes it; after that it continues until it is prevented by another body which touches it. Any other kind of operation on bodies is either miraculous or imaginary. (Lebniz, fifth letter to Clarke, Alexander (1956), p. 66.)

Thus, perceiving that the Newtonian argument was that the denial of the existence of both absolute space and causal action at a distance leads to an inconsistency, Leibniz postulated the reality of centrifugal forces—thereby denying one of the premises necessary to Newton's argument. He explained centrifugal effects not as inertial effects, but as the products of these real forces. Furthermore, it is these real forces which cause and sustain motions, so that the motion of the water in the bucket, as well as the shape of its surface, is an effect of the forces, thus opening up the possibility that, in a suitably impoverished universe, centrifugal effects may be present without a motion. (But cf. footnote 1 below). He even goes on to suggest (Alexander (1956), p. 74) that we may define '*A* is moving absolutely' as 'a cause of motion is present in *A*'. Then absolute and relative motions may be distinguished in such a way that all motions would be categorised in accordance with the corresponding categorisations of Newton. This theory of motion differs from Newton's in that it does not require the existence of absolute space, and that in it centrifugal effects are not due to absolute motions, although centrifugal effects and absolute motions always accompany each other.

The trouble with this position is that if accepted, it is not enough simply to reconstruct Newtonian mechanics in such a way that all of Newton's references to absolute space are replaced by references to physical systems: rather Newtonian mechanics would have to be rejected in substantial part. This is easy to see. According to Newtonian mechanics, a body moving in circular motion, e.g. a sling, is subject to the action of a centripetal force, directed towards the centre of the circle described by the stone. Presumably, Leibniz's centrifugal force would have equal magnitude to this centripetal force, but opposite direction, so that Leibniz must hold either that both forces exist, in which case the resultant force on the body is zero, or that centripetal forces do not exist, in which case there is no resultant force towards the centre of the motion. In neither case could the reconstructed Newtonian mechanics account for the motion. Leibniz did not develop his own mechanics sufficiently to warrant that his alternative theory be taken seriously.

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[1] It is unlikely that Leibniz accepted the thought-experiment of the globes as evidence for (*d*). For Leibniz would not have seen it as part of the function of a physical theory to have consequences regarding imaginary worlds. A physical theory need not entertain consequences incompatible with the principle of sufficient reason, and as Leibniz understood this principle, the world's constituents cannot be other than they are, for God had sufficient reason to make them the way they are, and no other way. In this conception of a physical theory, Leibniz anticipates Mach, who, of course, had a different ground for it.

4.6 There is a rather obscure argument presented by Berkeley which might be interpreted as an argument against (*d*).[1] Reconstructed in order to bring out its point, it is as follows: Newtonian mechanics does not need the existence of absolute space to be applied to the universe at large. All that is required is that there exists an inertial system, i.e. a set of objects with respect to which the Newtonian laws hold, or a set of bodies upon which no forces are acting. On this account, centrifugal effects occur whenever there is rotation with respect to an inertial system. Does this commit us to causal action at a distance? Consider Newton's thought-experiment. Consider first the globes and the fixed stars relatively at rest. As a matter of fact, the system of the fixed stars constitutes an inertial system (approximately). Now apply a force to the globes so that there is rotation relative to the stars. There will be a tension in the cord. If, on the other hand, a force is applied to the stars in order to generate the relative motion, there need not be a tension in the cord, for with the application of the force, the stars cease to be an inertial system and themselves accelerate with respect to any inertial system. Thence no prediction follows from Newtonian theory, since it ceases to be applicable within the system defined by the stars. The point of the argument is to suggest that a relational view can be held without the undesirable, counter-intuitive symmetry (that the effects are the same in the cord irrespective of the places of operation of the forces which generate the relative motion), and without action at a distance.

Certainly this is, on the surface, as good an interpretation of the thought-experiment of the globes as that proposed by Newton. However, if we reverse the order of the steps of the experiment as I did above, the present interpretation fails. Suppose that there is a tension in the cord. What happens if then the stars are annihilated? Then there would be no inertial system left, and no other body to which motion can be referred, so that the tension should vanish: thus causal action at a distance is not eliminated. One might avoid this conclusion by adherence to the principle that objects cannot be annihilated.[2] But this will not help greatly, for the point can be restated: the effect of certain forces applied to the globes will be a function of whether or not there are other objects in the universe. Thus, if we reject absolute space, we cannot reject causal action at a distance (unless we take Leibniz's objectionable option).

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[1] Suchting (1968), p. 193, refers to the possibility of this interpretation, and also to the unlikelihood of its being the correct interpretation. References to the relevant passages in Berkeley are cited by Suchting. Suchting also shows how certain other arguments of Berkeley fail to refute Newton's interpretation of the bucket-experiment.

[2] Berkeley, unlike Mach, had no objections in general to arguments which introduce the creation or annihilation of objects, nor to those which freely introduce applications of

4.7 If we accept causal action at a distance can we avoid commitment to absolute space? Suppose that we interpret Newtonian mechanics in the manner which I have attributed to Berkeley, but also accepting some action at a distance. Consider again the thought-experiment. We have seen that according to this interpretation the centrifugal effects are not simply a function of the relative motion between the globes and the stars, but a function of this relative motion when the stars constitute an inertial system. Suppose that there is a relative rotation and that this is due to forces acting on the stars, so that there is no tension in the cord, since the globes are not rotating relative to an inertial system. Suppose now that sufficient force is applied to the globes to bring them to the state of relative rest with respect to the stars. Then it would be hard to avoid the conclusion that the globes are rotating with respect to an inertial system, although, on the proper relational account, there is no motion in the universe. There is no motion in the sense that there is no relative motion among the physical objects of the universe, but there is motion relative to an inertial system, or at least this conclusion is hard to avoid. (I deliberately qualify the point in this way in order to avoid the lengthy digression that would be necessary to show that other interpretations of the present thought-experiment are unsatisfactory.) But what is this inertial system? It is not a set of physical objects. It is an ideal theoretical term. Absolute space? Not necessarily, for its role in the physical theory could be specified perhaps by reference to the behaviour of the objects of the universe. Nevertheless the absolutist overtones are there, since the natural explanation of why a system functions as an inertial system is that it is not accelerated with respect to absolute space. Such overtones will remain so long as inertial systems occupy a place of privilege in physical theory. Thus, in the long run, an intellectually satisfying relational theory of space requires the development of a theory that does not single out for special treatment any physical objects. To see this was Einstein's great insight. Mach saw where Newton's argument failed to be decisive: Einstein initiated the alternative theory that was needed to dethrone Newton.

The argument of the previous paragraph rests upon interpreting Newtonian mechanics in the manner which I attributed to Berkeley, and accepting some action at a distance, viz. that discussed in the second paragraph

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forces without reference to the sources of the forces, or to whether there are physically possible sources. See the texts cited by Suchting (1968), p. 189.

of **4.6**. But the complete symmetry of the radical relationalist position is not accepted. Alternatively, one can accept the complete symmetry, thereby accepting that the effects in the thought-experiments are a function of the relative motion alone, irrespective of the source of the relative motion and irrespective of any considerations relating to inertial systems. Then, if a force applied to the stars causes a relative motion, there will be a tension in the cord. But the stars no longer constitute an inertial system, so that the tension is due to a rotation with respect to a non-inertial system. Thus a Newtonian explanation cannot be given. To accept the complete symmetry is already to have deposed inertial systems from their position of privilege, and thence to have transcended Newtonian mechanics. The conclusion remains: that application of Newtonian mechanics to the universe at large carries a strong presumption of the existence of absolute space. Of course, this presumption disappears if we only wish to apply Newtonian mechanics to isolated systems.

4.8 It follows from the above discussion that any relational theory applicable to the universe at large adequate to account for the bucket-experiment must have the characteristics:

(1) That centrifugal effects are explained in terms of the relative motion of a body with respect to a framework determined by all the objects of the universe. (See **4.2**.)

(2) That there is causal action at a distance.

None of the relational theories proposed by Descartes, Leibniz and Berkeley possessed these characteristics, yet (1) is necessary to undercut Newton's first argument, (2) to undercut his second. Mach proposed a theory with both these characteristics.[1] Furthermore, Newtonian mechanics (in so far as it is applicable) may be applied to phenomena presently available to experiment or observation on the weaker condition:

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[1] It is interesting to note that, while Mach accepted (*a*) of Newton's argument, he challenged (*a'*) and (*b*), conjecturing that there are effects in the water due to the rotation of the bucket, but too small to be detected observationally, but which, he conjectured, might be observable if the bucket's walls were many thousands of times thicker. (Mach (1960), p. 344.) This conjecture, however, does not introduce a new factor into the discussion; it is merely an instance of his general thesis that all the bodies of the universe contribute towards the centrifugal effects. It is interesting to conjecture what Newton's response would be to an experimental detection of Mach's effects. If my interpretation of Newton is correct, such detection would not seriously damage his conclusion (given his premises). All Newton really requires is the premise: 'There exists a case of centrifugal effects which cannot be explained as a consequence of relative motion.' This can be derived from (*a*) and (*b'*); 'The shape of the surface of the water at equilibrium in stage 2 is not dependent upon the motion of the water relative to the bucket.' If my interpretation is correct, Newton's position is: 'The only possible relevant relative motion is that relative to the bucket', *not* 'the relative motion most likely to have an effect is that relative to the bucket'. Thus, Newton's use of (*a'*) and (*b*) is strictly unnecessary, though dialectically important.

(1′) That centrifugal effects are explained in terms of the relative motion of a body with respect to inertial systems defined relationally (as, e.g. in Smart (1968), p. 214).

But neither the existence of a relational theory with the above characteristics, nor the above-mentioned fact about the applicability of Newtonian mechanics, warrants the conclusion drawn, for example, by Smart:

> . . . that in the context of Newtonian mechanics the theory of absolute space and time is scientifically otiose. Newton could have said all that he really needed to say from a scientific point of view by talking of the necessity for determining inertial systems, and inertial systems can be discussed consistently within a relational theory of space and time. (Smart (1968), p. 216.)

Mach was right that the existence of a relational theory with the above characteristics is sufficient to show that Newton had not established his case; he was wrong that it is sufficient to show the scientific uselessness of absolute space.[1]

It is true that everything that is currently tested can be stated within a relational theory with the above characteristics. Equally, it can be stated within the absolute theory. Why then choose one rather than the other? The relational theory has the advantage that it is not committed to entities which are unobservable in principle; the absolute the advantage that there is no causal action at a distance. But to choose on the basis of either consideration is to choose on *a priori* grounds. In particular, choice on the basis of the first consideration amounts to an *a priori* limitation upon the kinds of entities which may be entertained in a scientific theory. (It is the Machians, not the Newtonians, who import metaphysics into science.) Since the theories have different consequences (even though not presently open to experiment) the issue between them is factual and not yet settled. There is nothing scientifically otiose about a concept whose employment leads to different predictions from those of theories which employ other concepts. To reject it out of hand, on philosophical grounds, may only be delaying the hour of reckoning.

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[1] Mach displayed a laudable caution in his acceptance of the relational theory. His rejection of absolute space was "provisional". He says:

> When we reflect that we cannot abolish the isolated bodies *A*, *B*, *C*, . . . , that is, cannot determine by experiment whether the part they play is fundamental or collateral, that hitherto they have been the sole and only competent means of the orientation of motions and of the description of mechanical facts, it will be found expedient provisionally to regard all motions as determined by these bodies (Mach (1960), p. 343).

However, even if subsequent observations made it necessary to reject the relational theory, he would be loth to embrace Newton's theory. He would prefer to postulate the existence of an all-pervasive medium, rotation with respect to which causes centrifugal effects. Regarding such a medium, "from the point of view of science it would be in every respect a more valuable acquisition than the forlorn idea of absolute space" (Mach (1960), p. 343). I fail to see the advantages of the medium. Granted that, unlike absolute space, it acts causally, still it is unclear whether it can be acted upon. If it cannot be, then the medium is at least as puzzling as absolute space; if it can be, the Newtonian thought-experiments could be performed with respect to it.

Furthermore, it is false that the relational theory asserts less than the absolute theory, since it is equally untested that inertial systems are to be explained correctly in terms of the distribution of matter in the universe, or in terms of their relationship to absolute space. Both theories involve departures from the currently testable in roughly equivalent degrees. No ground for choice can be found here. An alternative position is to remain uncommitted, just accepting that approximate inertial systems exist, and leaving it unexplained (i.e. accepting a theory with only (1′)).[1] Perhaps this is the most reasonable course to take, but this is not enough to show that absolute space is scientifically otiose. (Note also the tentative conclusion of 4.7.) Absolute space might not be required now, but if one can foresee possible circumstances in which it would be required and others which would be inconsistent with it, then it is well to be quite clear regarding what versions of it are scientifically viable. In addition, it is a perfectly proper scientific attitude to have firm commitment up to a certain point, and beyond that to entertain a plurality of conflicting theories.

It might be objected that I am exaggerating the possibility of future discoveries enabling a proper choice between the two theories. It is true that the two theories are not equivalent, in the sense that they have logically distinct consequences. But these consequences are such that no experimental or observational determination between them is possible, if the laws of Newtonian mechanics are correct. If this objection can be sustained, the proper position will be that of uncommitment; Newton's and Mach's positions will be equally objectionable, since each would be equally untestable. This is an important and natural objection to consider, since my discussion of the differences between the two theories was conducted mainly in connection with Newton's thought-experiments and variations of them. Since Newtonian mechanics contains the laws of conservation of mass and momentum, experiments involving creation and annihilation, or the acceleration of the whole frame of fixed stars, or forces which act on objects and originate from no physical object, are impossible in principle, if the Newtonian laws are correct. The thought-experiments, however, function as simple illustrations. While they cannot be performed or observed, there is nothing in the laws of Newtonian mechanics inconsistent with there being significant changes in the distribution of the matter of the universe. The two theories again entail different consequences in such an eventuality.[2]

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[1] Some passages in Mach suggest that this is actually his position. See the passages quoted in footnote 2 and footnote 2, p. 339.

[2] Cf. Mach:

> It is impossible to say whether the new expression would still represent the true

Thus, one kind of Newtonian thought-experiment may be considered as an illuminating, although oversimplified account of a physically possible identifiable situation, viz. that kind of thought-experiment which involves the acceleration of the frame of the fixed stars. Another kind, however, would be dismissed by Mach as "bereft of all scientific significance" (Mach (1960), p. 342), viz. that kind which involves annihilation and creation. Given Newtonian mechanics, there is no way in which experiments resembling such situations could be performed, so that there is no physically possible test.[1] Physical objects for Mach are like the points of absolute space for Newton: it is senseless to imagine their non-existence. Newton, on the other hand, saw the thought-experiments simply as revealing consequences of his laws, and, since he regarded his laws as scientifically significant, so too their consequences.

Newton and Mach had different attitudes to the thought-experiments because of a completely different conception of the nature of scientific laws. For Newton, his laws are about all the objects of the universe, and true of them at any time irrespective of their configuration and distribution throughout the universe. Furthermore, they hold of these objects even if some are annihilated or others created. They hold of any objects in any universe. The laws express physical necessities. Their applicability in a description of the universe does not depend upon contingent features of the universe, either regarding its constituents or the relationships among them. For Mach, on the other hand, laws concern correlations of experimental facts.[2] They are not true of all objects in general. Rather they apply to objects situated in specified frames of reference, and if such frames cease to exist the laws cease to have application. Thus the laws have no consequences regarding universes with different constituents, or even

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> condition of things if the stars were to perform rapid movements among one another. The general experience cannot be constructed from the particular case given us. We must, on the contrary, wait until such an experience presents itself. Perhaps when our physico-astronomical knowledge has been extended, it will be offered somewhere in celestial space, where more violent and complicated motions take place than in our environment (Mach (1960), pp. 347–8).

[1] In order to dramatise how broad the physical possibilities are, Professor Ellis suggested to me that our universe might be the two-globe universe. We might discover that the universe actually consists of two gigantic galaxies, joined at a fixed distance apart by something resembling a cord. This might break. If the galaxies recede, then they had been rotating in absolute space.

[2] He says:

> All our principles of mechanics are ... experimental knowledge concerning the relative positions and motions of bodies. Even in the provinces in which they are now recognised as valid, they could not, and were not, admitted without previously being subjected to experimental tests. No one is warranted in extending these principles beyond the boundaries of experience. In fact, such an extension is meaningless, as no one possesses the requisite knowledge to make use of it (Mach (1960), p. 341).

regarding this universe if the distribution of matter changes sufficiently radically. The laws express pure contingencies. Their applicability does depend upon contingent features of this universe, both regarding its constituents and the relationships among them. Newton allows far-reaching inductive inferences; Mach restricts us to close contact with the observed facts. Einstein, it is interesting to note, construed his laws in the Newtonian way, although his theory possesses the Machian characteristics.

The difference between Newton and Mach cuts very deep. Their differences regarding acceptable concepts reflect differences regarding their conceptions of scientific laws, and of the nature of science. But the issue is not merely one between Newton and Mach. It is one between absolutists and relationalists. Within the domain where Newtonian mechanics is the only existing mechanics, there are only two ways in which a relational theory can be entertained: the way which I attributed to Berkeley, with its absolutist overtones, and the Machian way. Without commitment to absolute space, then, the laws of Newtonian mechanics cannot be considered as unrestrictedly universal. Relational accounts of Newtonian mechanics are not compatible with one of the standard, if controversial analyses of the nature of scientific laws (unless, perhaps, Newton's laws are significantly reformulated).

4.9 My argument has been that although Newton's argument is unsound, experiment could settle the issue on the side of the absolute theory. That is, there are describable possibilities which, if they occurred, could not be explained by the relational theory. The outcome of experiment could drive us to the firm conclusion that there is absolute motion. Does the acceptance of absolute motion entail the acceptance of absolute space in the full Newtonian sense? Is it possible to have absolute motion without absolute space? This would be senseless to Newton since he defined absolute motion in terms of absolute space. But is there an alternative which does not do this?

Newton seems to get too much from his interpretation of the bucket-experiment. He infers an absolute acceleration, and so he is able to pick out a class of inertial systems, that is, a class of systems moving uniformly with respect to absolute space. But he cannot pick out a system at rest in absolute space. It seems then (since Newton does not consider any other phenomena which require absolute space for their explanation) that all he requires is the notion of 'absolute acceleration'. Even so, I fail to see how a satisfactory conceptual scheme could be developed in which 'A is absolutely accelerated' is not analysed as 'A is accelerated with respect to absolute space' (unless we accept Mach's position.) The one possibility seems to be: 'centrifugal effects are present in A', but such an analysis would undercut the Newtonian *explanation* of centrifugal effects. Still, it is not clear that Newton's full-blown absolute space would be needed. So far as I can see, the above argument seems only to require absolute space as a topological space, not as a metrical one. If so, the argument does not run counter to Grünbaum's argument that Newton's space cannot support an intrinsic metric. But I will leave these matters as queries. I also leave as a query whether the Newtonian characteristic of absolute space being incapable of entering into causal relations is needed, as well as that of its being ontologically independent of matter. When Grünbaum (1963, ch. 14) argues that the failure to implement Mach's Principle in the General Theory of Relativity involves the retention of absolute space, he understands a space with the latter, but not the former characteristic.

5 NEWTON may well be mistaken that absolute space exists, and developments connected with the theories of relativity suggest that he is. But the issue is a factual one, to be settled by empirical means. The assumption that absolute space exists is not redundant, since rival assumptions have different consequences which are potentially testable. Absolute space is an objectionable component of scientific theories *a priori* only if it is held that scientific theories should not extend beyond the presently testable. Thus the view expressed most sharply by Bridgman must be modified:

> Reflection on the situation after the event shows that it should not have needed the new experimental facts which led to relativity to convince us of the inadequacy of our previous concepts, but that a sufficiently shrewd analysis should have prepared us for at least the possibility of what Einstein did. (Bridgman (1927), p. 1.)

A sufficiently shrewd analysis would have posed a viable alternative, but it would not have settled the issue. It would not have shown the inadequacy of the Newtonian concepts.[1]

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Free Will in a Mechanistic Universe?

by P. T. LANDSBERG and D. A. EVANS

1 INTRODUCTION

In this paper we assume the brain to be a physico-chemical system the states of which can be described, and the future states of which can be predicted, in detail. We then inquire into the consequences of this assumption for the concept of free will. Consider, then, a human agent *A*. The states of his brain are being studied by a scientist *S* who is attempting to evolve a scientific description of *A*'s brain. This description, or model, it is hoped, will enable *S* to predict reasonably well future brain states from present ones. *S* has access to funds, research assistants, laboratories and computing facilities which are adequate for making sensible predictions.

Each of *A*'s distinguishable mental states including thoughts and emotions (i.e. his states of consciousness) will be supposed to correspond to a clearly defined set of physico-chemical states of his brain. We suppose further that *A* and *S* have agreed in a preliminary discussion on a language *L* in which any description of *A*'s consciousness is to be written. Any prediction made by *S* to *A* will be of the form 'at time t_2 your state of consciousness will be *P*', where *P* (with suffixes, etc. where needed) is a description in the language *L*, which *A* can check by introspection at t_2. An actual brain state will be denoted by '*B*'. The phrase 'brain state *B*′' will denote a description, within *S*'s theory, of a calculated or observed brain state. In a perfect theory there is a one-to-one correspondence between *B* and B′. For convenience the symbol '*P*' will be used to denote the prediction communicated to *A*, as well as the predicted brain state. It would be possible to consider a more detailed language *L*′ in which each *B* could be described uniquely. This would however raise the logical problem of whether a brain can hold a complete description of its own state. The coarser language *L* is probably adequate for elucidating the main logical points.

Next a mechanistic universe is assumed. This means we have to argue within nineteenth-century science, or in terms of a possible future mechanistic science. In this context a question of special interest is whether free

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We wish to put on record the stimulation we have received from the writings of Professor D. M. MacKay, and we are indebted to him and Dr. E. Cassirer for helpful criticism.

will is conceivable. In order to discuss this it is convenient to stipulate by way of definition that an agent A has free will in a certain situation if one of the following holds:—

D1 It is impossible *for any observer* to possess a complete and certain prediction of one of A's future brain states.

D2 It is impossible *to provide A* with a complete and certain prediction of one of A's future brain states.

D3 It is impossible *to provide A* with a complete and certain prediction of one of A's future brain states, *with which A would be correct to agree.*

We shall speak of "free will in the sense (D1), (D2), etc." Since logical difficulties will be seen to arise only for A, any complete and certain predictions will presumably be believed by all rational[1] observers (other than A) in all three cases.

As one passes from (D1) to (D2) to (D3) additional situations conforming to the definitions in question become available. The definitions are therefore in order of increasing weakness. The merits of these definitions are discussed in section **6.**

The definitions (D1) to (D3) can be described as objective in that they refer to the existence or non-existence of a prediction. A subjective definition would also be possible, and might take the form 'A has free will provided he does not *know* of any valid prediction of his future brain states' or 'A has free will if he *believes* that he has free will.' Such a definition if accepted would make the presence or absence of free will independent of physical determinism. We reject these definitions because they violate ordinary language. For instance they would ascribe free will (with its corollary, moral responsibility) to a person acting under a rigid post-hypnotic suggestion of which he is unaware.

We shall now formulate three parallel questions which we wish to discuss in this connection:

Granted a mechanistic universe, is it conceivable that a scientist S can at time t_1

Q1 *possess* a complete and certain prediction of A's brain state at a later time t_2, *on which S and third parties* would be correct to agree at time t_1 and subsequently?

Q2 *provide A* with a complete and certain prediction of A's brain state at a later time t_2, *on which S and third parties* would be correct to agree at time t_1 and subsequently?

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[1] In this paper, unless otherwise stated, all persons will be assumed to be "rational" in the sense that they are able to understand, and think logically about, scientific statements; that they are not hypnotised or in other exceptional mental states. This is not of course intended to be a full definition, but merely an indication of what we intend.

Q3 *provide A* with a complete and certain prediction of *A*'s brain state at a later time t_2, *on which S, A and third parties* would be correct to agree at time t_1 and subsequently?

Observe that

'Yes' to (Q3) implies 'yes' to (Q2) and (Q1)
'No' to (Q2) implies 'no' to (Q3).

We see that an answer 'no' to (Q*r*) implies the existence of free will in the sense (D*r*) ($r = 1,2,3$) even in a mechanistic universe. An answer 'yes' similarly establishes the logical consistency of a universe in which all human beings lack free will in the sense (D*r*). In this paper an answer 'yes' is suggested to all three questions. We shall spend no time on (Q1) since the answer 'yes' is almost tautological in view of the meaning of the word 'mechanistic', and we know of no disagreement with this answer.

We have added (Q1) so as to provide us with a means of emphasising some special features of (Q2) and (Q3). The latter imply communication with *A* about his own future brain state and this leads to complications which need rather delicate analysis.

The analysis of these questions is simplified by the consideration of four distinct situations (*a*) to (*d*) introduced in 2. If case (*a*) holds it enables (Q2) and (Q3) to be answered by 'yes'. If cases (*b*) and (*c*) hold, the answer 'yes' can be given to (Q2).

If case (*d*) applies to a certain situation, then, for this situation, free-will of type (D2) and (D3) has been established. It is still conceivable, however, that, in other situations, one of the cases (*a*) to (*c*) will apply. Hence the answer to questions (Q2) and (Q3) need not be 'no' on the strength of a limited number of situations of type (*d*). It can be 'no' only if all conceivable situations fall into category (*d*).

Our answer will be 'yes' to all three questions: the possibility of one case, say case (*a*), is sufficient to enable us to give such an answer.

That our answer is 'yes', is not very interesting perhaps, or very surprising, except for two considerations. The first is that the answer 'no' has implicitly been given by Professor D. M. MacKay (1965, 1967) to (Q2) and (Q3), and this appears to have been widely accepted (Thorpe, 1969) for a number of years. Secondly, the argument leading to this 'no' is interesting and quite involved and it is not easy to find what we hold to be the error in it.

The answer 'no', if it were correct, *would* be useful. For one could go on to note that if freedom of action exists even in a *mechanistic* universe, how much more secure is it in this our *real* universe, in which the quantum laws ensure that the causal chains are less tight. A large area of the arguments between determinists and their opponents would be cleared, and philosophers would certainly want to take more note of this reasoning than they appear to have done. The determinists would have to admit a serious defeat. The answer 'yes' which we propose, has no such interest. Nothing follows from it about the real universe and the main interest of our considerations must therefore lie in the dissection of certain logical difficulties which is offered here. But we may perhaps claim that, if we succeed here, the whole range of the free will—determinism confrontation is again open to philosophical speculation.

2 FORMAL STATEMENT OF THE POSSIBILITIES

It is not our purpose to discuss the existence or otherwise of free will, nor is it our purpose to discuss whether or not the future state of a live human brain is determined by its present state. These important matters are beyond our scope. We are solely concerned to enquire what interesting inferences may be drawn from a purely mechanical picture of the human brain. Although we speak about brain states in this paper it will be assumed that full information about A's *body* is available to the scientist S.

The agent A is supposed to be isolated from all stimuli except those known to S in a period of time t_0 to t_2. He is located in a windowless soundproof room containing a teletype for purposes of communication. We suppose that A's brain state at t_0 is $B(t_0)$ and that S has a correct and detailed description $B'(t_0)$ of this state. He is asked to predict the state of A's brain at the later time t_2. At an intermediate time t_1 A will be informed of the predicted state, and A will believe or disbelieve the assertion made by S. Suppose that, between t_0 and t_1, S arrives at the prediction that A's brain state at t_2 will be $B'(t_2)$, and suppose that the actual state of A's brain at t_1 is $B(t_1)$. Next a description $P(t_2)$ of the predicted brain state $B'(t_2)$ is communicated by teletype to A. This changes A's brain state from $B(t_1)$ to $B^+(t_2)$ or $B^-(t_2)$ at the later time t_2. $B^+(t_2)$ occurs if A believed the prediction at t_1, $B^-(t_2)$ if A did not believe it. Other possibilities could be envisaged, but we shall here avoid the additional complications resulting, for example, from the use of *n*-valued logic (Landsberg (1953), p. 379; (1954), p. 14). This results in a situation which can be represented as in Fig. 1. We need to consider four possibilities of special interest:

(*a*) $B'(t_2) \doteqdot B^+(t_2)$.[1] In this situation A is right if he believes the prediction at t_1, and may or may not be right if he disbelieves it.

(*a* i) If A believes the prediction, his brain goes over into the predicted state, and he was right to believe the prediction.

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[1] This and similar approximate equalities between predicted and actual brain states can be interpreted as meaning that the descriptions in language L of the conscious states corresponding to the two brain states involved are identical.

(*a* ii) If A disbelieves it, his brain state at t_2, $B^-(t_2)$, may or may not be one of the many detailed states consistent with the prediction $P(t_2)$. This uncertainty is due to the coarseness of the language L. If it is consistent with $P(t_2)$ the prediction is in fact true and A was wrong to disbelieve it. If, however, $B^-(t_2)$ is inconsistent with $P(t_2)$, the prediction is wrong and A was right to disbelieve it. MacKay (1967) in a less detailed analysis seems to detect a freedom of the will here, since the two opposite courses (*a* i) and (*a* ii) can both make A's view correct.

(*b*) $B'(t_2) \doteqdot B^-(t_2)$. In this situation A is wrong if he disbelieves the prediction at t_1, and may or may not be wrong if he believes it.

(*b* i) If A disbelieves $P(t_2)$, $P(t_2)$ becomes a correct prediction and A is wrong.

(*b* ii) If A believes $P(t_2)$, his brain state at t_2, $B^+(t_2)$, may or may not be consistent with $P(t_2)$. If it is, he is right; if not, he is wrong.

In (*a* ii) and (*b* ii) above, the formal possibility is envisaged that a prediction $P(t_2)$ may be correct although the calculation leading to it is incorrect. The probability of such "accidental" correctness would be expected to be very small in a complex system such as the brain. If this probability is neglected, cases (*a*) and (*b*) can be summarised more simply:

In case (*a*), A is right whether or not he believes the prediction.

In case (*b*), A is wrong whether or not he believes the prediction.

This simpler situation is assumed in discussing A's free will in 4.

The fact that there is interaction between A and S, and that this depends on the brain state of the scientist himself, may lead to the possibility of a number of predictions, or indeed none. Interpreting cases (*a*) and (*b*) as referring to situations in which the theory yields one, and only one, prediction, one must introduce two more cases:

(*c*) Several predictions exist, each prediction considered alone being of type (*a*) or (*b*).

(*d*) The theory yields no self-consistent prediction.

These cases will be discussed more fully in the next section.

It is part of our model that the communicated prediction $P(t_2)$ shall not contain any reference to A's acceptance or rejection of this prediction at t_1. If A's predicted state of consciousness at t_2 contains memories of this acceptance or rejection, descriptions of such memories are to be excluded from $P(t_2)$. By this exclusion we avoid the possibility that case (*b*ii) may involve A in a self-contradiction. However, since we later rule out (*b*ii) as physically impossible, inclusion of such memories in $P(t_2)$ would not significantly alter our main conclusions in section 4.

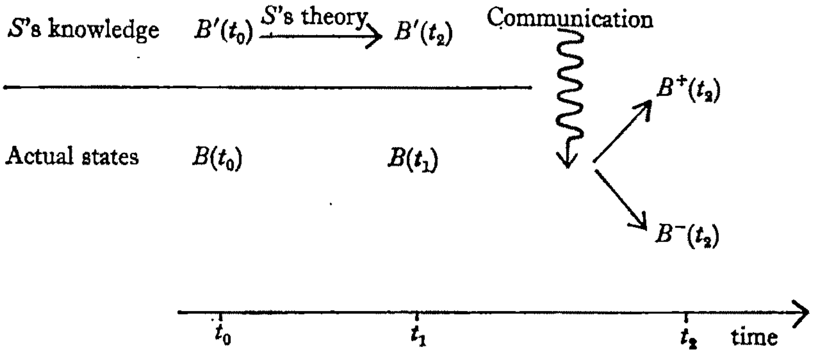

Figure 1

3 THE PROBLEM OF OBTAINING A SELF-CONSISTENT PREDICTION

We have assumed that A's brain state at time t_0 is known, and that all influences acting on A's brain in the time interval (t_0, t_2) are likewise known in advance, with the exception of a prediction P to be communicated to A at t_1. We also assume that, given P, A's brain state at t_2 can be calculated with certainty from the data, and that the calculation can be repeated many times in the interval (t_0, t_1). The assumption is not unreasonable: modern electronic components can operate a million times faster than a human neuron, so that a sufficiently large electronic simulator could run through a ten-second period (t_0, t_2) of A's brain states one thousand times in one hundredth of a second, assuming each neuron is represented in the simulator by one electronic component.

These assumptions allow us to define a function $f(P)$ as follows: $f(P)$ is the description, in language L, of the brain state at t_2 calculated on the assumption that P is communicated to A at t_1. The form of $f(P)$ depends of course on the nature of A's brain. In addition it depends on the times t_0, t_1, t_2 and on A's brain state at t_0, and these are considered as constants. The equation $P = f(P)$ then asserts that the description of the brain state resulting from this procedure is itself P, so that a prediction P satisfying this equation is self-consistent. We are therefore looking for a solution $P = P(t_2)$ of this equation. One computational procedure for finding such a solution would be an iterative method, defined as follows.

(i) Calculate the brain state expected at t_2 if no prediction is communicated to A at t_1. Call this brain state '$B'_1(t_2)$' and its description 'P_1'.

(ii) Calculate the brain state $B'_2(t_2)$ expected at t_2 if P_1 is communicated to A at t_1. Let the description of $B'_2(t_2)$ be P_2, so that $P_2 = f(P_1)$. Calculate in the same way a sequence of states $B'_r(t_2)$ with descriptions $P_r(r = 3,4, \ldots)$, $B'_r(t_2)$ being the state expected at t_2 if P_{r-1} is communicated at t_1, so that $P_r = f(P_{r-1})$. Continue until a state $B'_m(t_2)$ is found such that $P_m = P_{m-1}$. Then P_m is the required prediction $P(t_2)$, and $B'_m(t_2)$ is the corresponding brain state $B'(t_2)$.

The procedure envisaged closely parallels one method of solving an equation $f(x) = x$ in the single algebraic variable x. It is subject to the same difficulties as its mathematical counterpart. For instance it may fail to find a solution which exists. More sophisticated mathematical techniques can of course overcome such problems, and one may hope that some suitable iteration technique would be able to find all solutions of $f(P) = P$.

The analogy with a mathematical equation at once brings to mind the possibility that the equation $f(P) = P$ may have no solution (case (*d*)), or more than one solution (case (*c*)). The case of no solution might arise if A and S had agreed at time $t < t_0$ that, at time t_2, A would be thinking either of an object O_1 or of an object O_2, and A were determined in the interval (t_1, t_2) to *falsify* whatever predictions were communicated to him at t_1. In this case A would respond to a prediction including the statement 'at t_2 you will be thinking of O_1' by thinking of O_2 at t_2, and conversely. One could then divide the possible predictions P into disjoint sets S_1, the set of all P's which are consistent with A's thinking of O_1, and S_2, the set of all P's which are consistent with A's thinking of O_2, and one would expect that if $P \in S_1, f(P) \in S_2$ and conversely, so that the successive P's in the above procedure oscillate indefinitely (Fig. 2).

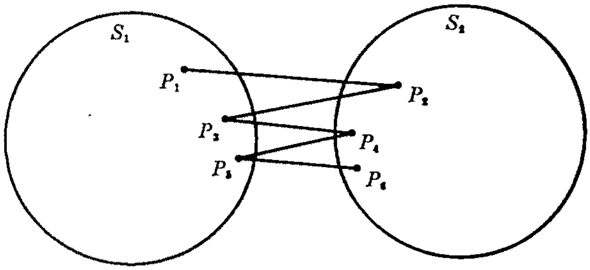

FIG. 2. First steps in the iteration process for a case in which no self-consistent predictions exist.

The case of more than one solution might arise in relation to the sets S_1, S_2 in several ways. If A desires in the interval (t_1, t_2) that the theory used by S shall be vindicated, *or* if he already has complete confidence in the theory, he will presumably choose to think of O_1 if $P \in S_1$, and of O_2 if $P \in S_2$. It follows that if the first approximation P_1 is in S_1, then $P_r \in S_1$

for all r, and similarly for S_2, so that the iteration procedure starting from a prediction P_1 in either set would lead to a final prediction $P(t_2)$ in that set. Two solutions could also arise if A is determined during the interval (t_1, t_2) to falsify S's theory. Suppose A believes in the interval (t_1, t_2) that S is lying to him and that when S communicates a prediction $P \in S_1$, the real, uncommunicated prediction P', say, which S believes, is in S_2. A's desire to falsify P' then has the result that if $P_1 \in S_1$, then $P_r \in S_1$ for all r; and similarly for S_2, so that two solutions again result (Fig. 3).

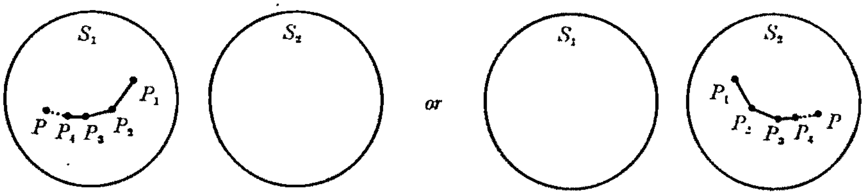

FIG. 3. First steps in the iteration process for a case in which two self-consistent predictions exist.

Here and in the rest of the paper phrases such as 'A is determined', 'A would choose', and 'A desires' are used. It must be clearly understood that they are not intended to prejudge the existence of free choices for A. They are only meant to convey the nature of certain subjective states of A's mind. The brain state at any time $t(t_1 < t < t_2)$, and hence the subjective state of mind at that time is on any mechanistic theory fully determined by $B(t_0)$, $P(t_2)$ and any other physical stimuli affecting A's brain in (t_0, t). The freedom of choice suggested by these terms may or may not be illusory, depending on the agent and on the definition (Dr) employed (see **4**).

The following example illustrates types of behaviour of the equation $f(P) = P$, but is not essential to the argument, and may be omitted by the non-mathematical reader.

A simple example illustrating some of the difficulties discussed above is provided by the function $f(x) \equiv ax$. Then the successive approximations to a solution of $f(x) = x$ are

$$x_1,\ x_2 = ax_1,\ x_3 = a^2x_1,\ x_4 = a^3x_1, \ldots$$

Thus if $-1 < a < 1$ the procedure converges correctly to the solution $x = 0$ (corresponding to our cases (a) and (b)). But if $|a| > 1$ there is no convergence even though the solution $x = 0$ is still correct. If the value $x = 0$ is for some reason not included among the allowed values of x, and $a = -1$, then one finds oscillations, but no solution (case (d)). On the other hand if $a = +1$, more than one value of x (in fact, every value) is a solution (case (c)).

4 THE FREEDOM OF THE WILL

The logical possibilities (*a*) and (*b*) discussed in 2, and the difficulties of prediction discussed in 3, suggest an indeterminacy resulting even from a mechanistic brain theory which could leave room for a freedom of the will. In this section these possibilities will be considered. We shall assume that S's calculation of A's brain state at t_2 enables him to follow the development of A's brain state in time from t_0 to t_2, so that the theory predicts, among other things, whether or not A will believe the communicated prediction P at any time t $(t_1 < t \leqslant t_2)$.

Suppose that the theory used by S predicts a unique brain state $B'(t_2)$ for A at t_2, and also predicts that A will believe the description $P(t_2)$ of this state when it is communicated to him at t_1. *This is case* (*a*) of 2, with a unique prediction. Then if A were ***not*** to believe $P(t_2)$ at t_1, his brain state at t_2 would not be the end result of the causal chain from time t_1 to t_2 considered by the theory, and therefore would not, except by an improbable coincidence, be one of those described by $P(t_2)$. A may therefore consider, at times between t_0 and t_1, that he is free to accept or reject the prediction to be communicated to him at t_1, since he would be justified by the event in either case.

The argument here attributed to A rests on a misconception. The *logically possible* event, that A might not believe $P(t_2)$ at t_1, is *physically impossible,* since it contradicts a prediction of a valid physical theory. Thus A is deluding himself prior to t_1 in judging as real the possibility of not believing $P(t_2)$ at t_1, and his supposed freedom to do so is illusory. In fact, only case (*a* i) is *physically* possible: A will believe the prediction and will be correct in doing so. *A does not have free will in any of the senses* (*Dr*) of 1 ($r = 1,2,3$).

Suppose next that the theory predicts a unique brain state $B'(t_2)$ for A at t_2, and also predicts that A will ***not*** believe the description $P(t_2)$ of this state when it is communicated to him at t_1. *This is case* (*b*) of 2, with a unique prediction. Then if A ***were*** to believe $P(t_2)$ at t_1, his brain state at t_2 would, as above, almost certainly not be consistent with $P(t_2)$. The theory can in this case not provide A with a prediction which he would be justified in believing at t_1, so that *A has free will in sense* (*D3*). As explained in the preceding paragraph only the alternative (*b* i) is *physically* possible: A will disbelieve the prediction and will be wrong in doing so.

Suppose next that the theory gives rise to several predictions $P_i(t_2)$ ($i = 1,2, \ldots$), each of which will correctly describe A's brain state at t_2 if it is communicated to A at t_1. *This is case* (*c*) of 2. There is an apparent indeterminacy here, but it is not located in A's brain which by hypothesis reacts determinately to stimuli supplied to it by S. The indeterminacy is in S's choice of which prediction to communicate, and is present only because S's brain state at time t_0, and other stimuli affecting him in (t_0, t_1), are unknown. S could remove the indeterminacy by deciding in advance on a pre-arranged rule (e.g. alphabetical order in the language L) which will choose one of the predictions $P_i(t_2)$ as the one to be communicated to A. The choice being made, one has either case (*a* i) or (*b* i).

Suppose finally that there is no prediction which will be correct at t_2 if communicated to A at t_1. *This is case* (*d*) of **2**. It will be assumed that this arises from A's conscious intention to falsify the prediction. That such an intention can lead to case (*d*) has been shown in **3**. Two cases can be distinguished:

(*d* i) arises if A has decided before time t_0 always to act in a sense opposite to the prediction. This will be known to S from his study of A's brain. As a result, for any prediction $P(t_2)$ communicated to A by S, S will be able to predict the actual brain state $B(t_2)$ of A at time t_2 consequent upon A's desire to falsify $P(t_2)$. Hence there exists a correct prediction $P^*(t_2)$ $[\neq P(t_2)]$ say, which S can make, and which he can validly communicate to all third parties, but not to A. *A therefore has free will in the senses* (D2) *and* (D3).

(*d* ii) A may be aware of the above considerations and seek to falsify both $P(t_2)$ and $P^*(t_2)$ by varying his response "at random". But how can he generate randomness? If he tosses a coin, his decision to do so and his precise actions are all predictable by S, who will therefore know the outcome of every toss. A similar argument will apply to other methods. The apparent randomness having been removed, case (*d* ii) becomes equivalent either to (*d* i) or to one of the earlier cases, depending on the outcome of the apparently random choice.

Table 1 summarises the findings of this section. Note that case (*c*) reduces to case (*a*) or to case (*b*) if the scientist S uses a definite rule for choosing between valid predictions. Similarly case (*d* ii) is equivalent to one of (*a*), (*b*), (*d* i) once the scientist has calculated the outcome of what appears to A as a random choice.

That the existence of free will (in any of our three senses) in a mechanistic universe cannot be asserted rests therefore largely on case (*a*), and the argument that this is an exceptional case needs now some consideration. It may be admitted that in our present society many people, perhaps a majority, would try to falsify any prediction of their own future brain state which was communicated to them. Their motive might be a desire to assert their own freedom, or simple curiosity. We would

Table 1 *Summary of situations considered*

	Exists in cases			
	(*a* i)	(*b* i)	(*d* i)	(*a* ii) (*b* ii)
(D1) free will	No	No	No	Physically impossible
(D2) free will	No	No	Yes	
(D3) free will	No	Yes	Yes	

consider however that any person might on rational consideration of the scientific evidence, and without any external compulsion, decide to believe all such predictions from a suitably qualified source. Such a person, for whom case (*a*) would be a normal occurrence, would have as much claim to be considered a free (in the everyday sense) and responsible agent as one who decided always to disbelieve or oppose the predictions, and should certainly not be considered exceptional, yet he would lack the free will in sense (D2) and (D3) possessed by the latter.

5 EARLIER ARGUMENTS IN FAVOUR OF FREE WILL

a. *General discussion*

The conclusions presented here differ from those derived by MacKay, as has already been noted in 1, where the importance for philosophy of finding the best answer to the title question of this paper was also emphasised. The notion of a *mechanistic universe* being perhaps not unduly controversial, we must therefore return to the definitions of *free will* (D1) to (D3). Of these (D3) was found to allow the largest number of human situations to furnish evidence in favour of free will; yet even with this definition we found it logically consistent to contemplate a mechanistic universe in which all human beings lack free will. How does MacKay (who does not formalise his definition but implicitly confines himself to a definition somewhat similar to (D3)) arrive at the opposite conclusion?

In the common-sense definition (D1) (in favour of which we shall argue in the conclusion) the agent A occurs once, in (D2) he is mentioned twice, and in (D3) three times. The element of self-reference inherent in (D3) and (Q3) leads to a complication which is not present in (Q1) and (Q2). This has led MacKay to argue that it is in fact impossible to be *certain* of the prediction envisaged under (D3), and he finds (D3) free will in a mechanistic universe.

Actually, MacKay argues for freedom of action rather than free will, but, neglecting the cases of a completely paralysed person, brain damage, hypnosis, etc., this difference is not significant in the present context.

b. *Discussion of specific points*

MacKay argues that a man's actions are causally dependent on his earlier brain states. Hence given the prediction based on a valid theory

$$P\colon A \text{ will do } X \text{ at } t_2$$

there will be necessary conditions for the validity of P regarding earlier influences, etc. Suppose one of these has the form

either C_1: A believes P at t_1
or C_2: A disbelieves P at t_1
or C_3: A is ignorant of P at t_1
or C_4: A knows about P at t_1 but does not believe it or disbelieve it.

If C_1 applies:

> If it is necessary for A to believe the prediction in order that it be confirmed, then A would not be in error if he disbelieved it, since it would then fail. Hence belief and disbelief are equally justifiable attitudes for A. The 'prediction' has no selective information content for him (MacKay (1965b), p. 265).

This is very similar to our case (*a*) of 2. We disagree with this evaluation in that we regard P as certain, although P would be false if A did not believe it at t_1. We do so simply because A's belief of P at t_1 is in fact completely determined by previous conditions, and if he, before t_1, considers as real the possibility of not believing P at t_1, he is deluding himself. His subjective feeling of freedom rests on the false proposition 'It is possible for me to disbelieve P at t_1' and is therefore illusory.

This disagreement can be traced back to a difference in the use of the word '*certain*' to which we referred under 5a above. MacKay here implicitly uses the definition

Definition of certainty: A proposition is certain if and only if any rational person would be right to believe it and wrong to disbelieve it:

> The word 'certain' is normally used to distinguish those statements that anyone would be in error to *disbelieve*, and correct to *believe*. (MacKay (1965b), p. 259.)

He argues: since A is rational and would not be in error to disbelieve P, P is not certain.

Our reasons for rejecting the above definition of certainty can be seen by reference to a different proposition. Let P be a present tense description of A's consciousness, of the form 'A believes Y'. If P is true, A knows the truth of P by introspection, and S knows it from his instrument readings. Now if A believes Y, he must logically (if the question occurs to him) also believe that he believes Y, i.e. he must believe P. Conversely if A were to disbelieve P, he would not believe Y and P would be false. Thus P, if true, does not satisfy MacKay's criterion of certainty. We consider on the other hand that P, if true, is certain, since A and S both have the best possible evidence for its truth. We therefore reject the suggested criterion.

Suppose now that C_2 applies. Then if A were to believe P at t_1, C_2 would fail, P would be false and A would have been wrong to believe P.

> The question is whether A is in error because he does not believe P, and the fact is that A would be in error if he did believe P... his nonbelief of P... *is not rendered erroneous by the facts that justify our own certainty of P.* (MacKay (1965b), p. 259).

This is very similar to case (*b*) of **2**, so that A has (D3) free will at time t_1. We concur with MacKay's view that in this case no prediction exists which could claim A's assent at time t_1. This circumstance is, in MacKay's view, sufficient to establish that A has freedom of action.

The freedom of action envisaged here is somewhat strange since A will in fact do X at t_2. The (D3) definition of free will is therefore not one that appeals to us. We prefer to regard (D2) as the weakest useful definition and to note that, as discussed in **4**, A does not have (D2) free will in case (*b*). This has the consequence that we describe a prediction P as certain, although A would not be correct if he believed P. We do not regard this difficulty as insuperable since it arises only in the case where what A in fact believes (the negation of P) is false. A, by rejecting an inference (P) drawn from a valid physical theory and correct data, has put himself into a position where he can have no correct opinion concerning P. We do not think he can be said to have acquired freedom of action, in any useful sense, by doing so, since he *will* do X at t_2.

If C_3 applies, P can be expressed as 'A will do X provided he does not hear of this prediction', thus making the condition C_3 explicit. Such a prediction can have no definite truth-value for A at t_1, since A cannot even consider it without violating C_3. For A, at time t_1, P is "logically indeterminate" (MacKay (1960), p. 34), and is not certain according to the above definition. Williams (1961) considered, on the other hand, that a prediction of the form 'A will do X if he does not hear of this prediction' can have the same truth-value for all observers including A, provided it is re-worded for A's consideration as 'A would have done X if he had not heard of this prediction'. In order to avoid these complications we have considered (except for the prediction P^* (t_2) referred to in discussing case (*d*i)) only predictions which A is intended to hear at t_1.

It is not expected that much insight will be gained by the consideration of C_4, which is therefore omitted.

c. *The two views*

We have in this section advanced a view which asserts the absence of free will in a mechanistic universe. In order to do so one must regard the proposition P (A will do X) as certain although, (i) in the case of C_1, A would not be wrong if he were to disbelieve P; and (ii) in the case of C_2, A would be wrong if he were to believe P.

MacKay's alternative view is that P is *not* certain under these conditions, and that A has therefore freedom of action; he concludes that this applies to all rational persons. To do so he must regard A's future action as free, even though that action is physically determined and known in advance to others.

6 CONCLUSION

In 1 we put forward three possible definitions of free will (D1, D2, D3). In subsequent sections we explored possible outcomes of an attempt by a scientist in a mechanistic universe, possessed of all relevant knowledge, to predict the future brain state of a subject and to communicate this prediction to the subject. We concluded that there exists a well-defined class of situations (our case (*a*), see end of 4) which is not exceptional and in which free will does not exist in any of the senses (D1, D2, D3). In sense (D1) free will is absent for all subjects. In sense (D2) and (D3) free will is present for some subjects and absent for others. But it should be emphasised that the subjects for which (D2) and (D3) free will are absent are not necessarily less rational than those who have this free will. Since we reject any weaker definition of free will, we must conclude that in a mechanistic universe, free will is not logically necessary.

A stronger conclusion follows if one rejects also definitions (D2) and (D3) as being too weak to do justice to the accepted meaning of the term 'free will'. One is then left with (D1), which is equivalent to the "common sense" view that any subject can be shown to lack free will by consistently predicting his brain states in advance, even if these predictions are not communicated to the subject. Since the theoretical possibility of such prediction is implicit in the meaning of the term 'mechanistic', one has to conclude that in a mechanistic universe free will is logically impossible. We have introduced (D2) and (D3) in order to give a careful discussion of MacKay's views. It must be pointed out, however, that these definitions might be rejected by some persons in favour of (D1). One might argue as follows:

In order to discuss the validity of (D2) and (D3), consider a computer which is equipped to handle messages describing its own internal state.

Such messages need not be at all detailed: a typical message might be of the form 'memory units 1 to n and logic units 1 to m are in operation; all other units are not in operation'. The state so described would be symbolised as (n, m). The message could itself be handled by logic unit 1 and stored in memory unit 1, so that no problems of information capacity arise. Now suppose that the computer is programmed to respond to any message of the form 'in r seconds you (the computer) will be in state (n, m)' by shutting down memory unit n (if $n \neq 0$) and keeping this unit shut down for $(r+1)$ seconds. If $n = 0$ it would be programmed instead to keep memory unit 1 in operation for $(r+1)$ seconds. This computer now has the properties (i) that no correct prediction of its future state can be communicated to it in advance, and (ii) that by any operational definition it is capable of understanding such predictions, i.e. of responding appropriately to them. The computer, taking the place of A, would therefore conform to definitions (D2) and (D3), so that to accept those definitions as valid is to ascribe free will to the computer. Some might consider this as absurd and would therefore reject these definitions. Although we are inclined to share this view, our purpose here has been merely to note our three definitions and pursue their implications.

It remains to make a remark about MacKay's Principle of Logical Indeterminacy (1967, p. 12). Its content can be indicated by the observation that an agent A cannot make "a prediction of his future brain state" and also "that there exists no definitive prediction that could claim his assent". For him "the only logically admissible view is that this particular future event is as yet *indeterminate*". In the course of the preceding analysis we have given the answer 'yes' to (Q3) and it follows that we cannot accept the Principle of Logical Indeterminacy as valid in a mechanistic universe.

In conclusion we wish briefly to criticise two assumptions which are made in this paper. Though this criticism does not affect our results, which we sought to establish in a clearly defined setting, the criticism is needed in a broader context of philosophy. The assumption of a mechanistic universe is clearly subject to criticism, but this is not the place to discuss the matter.

One can also be critical of the assumed existence of a language L. Although it may one day be constructed, much simpler problems of this type are still unsolved. For example, one wants to describe thermodynamic states ("macro-states") of systems in terms of the many ($\sim 10^{23}$) micro-states to which each macrostate corresponds. This is achieved by the introduction of macro-observables (Landsberg (1961), p. 463) but a detailed and general procedure of finding these from micro-observables is not yet known. The construction of a language L is faced by the same

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Is Quantization Really Necessary?

by M. SACHS

SINCE THE INITIAL SUCCESSES of quantum mechanics, it has been generally assumed that the features that underlie the mathematical structure of this theory are necessitated by the data of experimental atomic and nuclear physics. While not all of the notions having to do with the Copenhagen interpretation of the quantum theory are considered by every physicist to be essential, those ideas that are nearest to the mathematical representation of the theory have been held to be sacrosanct by the adherents and (most of) the heretics alike.[1] I should then like to discuss in this paper the question of whether these particular notions dealing with the aspects of quantum mechanics that almost everyone seems to agree with are in fact essential to explain the apparently discrete properties of matter in the microscopic domain.

1 BEFORE EMBARKING on the discussion of the quantum theory itself, I should like to point out an important distinction in theoretical physics that is not always made by physicists or philosophers of science. This is the difference between a physical theory and its mathematical representation. When I use the word 'theory', I refer to an hypothesis (or a set of hypotheses)—the abstract conceptual foundation of natural phenomena. On the other hand, within the framework of theoretical physics, mathematics is not more than a language that is used to facilitate, most economically, a representation of the theory that would permit precise statements and predictions to be made. This distinction between theory and mathematics (i.e. between concepts and language) is important to note because the special mathematical representations that scientists use to explore the theory are not always unique to that theory. For it has happened in the history of physics, and it will undoubtedly continue to happen, that the mathematical representation of a theory which provides an accurate description of some particular phenomena, ceases to be accurate as the

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I should like to thank the elementary particle physics group in the Department of Applied Mathematics and Theoretical Physics, University of Cambridge, for their hospitality during the Michaelmas term, 1969, when this article was written.

[1] An example of the heretical stand that does not abandon the underlying mathematical features of quantum mechanics, but rather adds to them, is the hidden variable approach of D. Bohm (1952). A recent summary of this approach was given by J. Bub (1969).

features of these phenomena are explored further in experimentation. In the latter cases, the earlier mathematical formalism often becomes an approximation for some different formalism which evolves from a theory with an entirely different conceptual basis. These occurrences will be exemplified in the discussion of the main theme of this paper which questions the conceptual basis of the quantum theory in view of its failure thus far to extend satisfactorily so as to be compatible with the theory of special relativity.

2 IT ALSO HAPPENS often that two (or more) mathematical representations can express the same physical hypotheses. Some of these can be transformed into each other. In these cases, from the point of view of physics, they would be equivalent expressions of the theory—the choice of using one of these expressions or the other would depend only on which is the most convenient for the problem at hand. However, some of the mathematical representations may not transform into the others. In the latter case, one of the representations would be more general than the others—it would make predictions that are out of the domain of the other mathematical formalisms, in addition to making predictions that are in one-to-one correspondence with those of the less general formulations. In these cases, the additional predictions may be implicit in the less general formulations and would not become explicit in the latter expressions until they were "unfolded". In other cases, however, the features of the more general formulation may contain aspects that could never be uncovered from less general formulations because additional undue restrictions may be built into the mathematical structures of the latter.

A well-known development in theoretical physics that exemplifies this point is Dirac's formulation for the electron equation from a factorisation of the Klein-Gordon equation. Recall that the latter equation was set up by using Schrödinger's wave mechanical prescription in the special relativistic formula for the energy of a free particle:

$$E^2 = p^2c^2 + m^2c^4 \qquad (1)$$

where p is the particle's momentum, m is its mass (in a rest frame) and c is the speed of light in a vacuum. Inserting Schrödinger's wave mechanical operator equivalents

$$E \to \hat{H} = i\hbar\partial/\partial t, \qquad p_j \to \hat{p}_j = -i\hbar\partial/\partial x_j \qquad (j = 1, 2, 3)$$

and allowing both sides of (1) to then "operate on" the elements ϕ of a linear function space, the latter equation of classical relativistic mechanics is converted into the Klein-Gordon equation:

$$\left\{\Box + \left(\frac{mc}{\hbar}\right)^2\right\}\phi = 0 \tag{2}$$

where

$$\Box \equiv \partial^2/\partial(ct)^2 - \nabla^2$$

is called the D'Alembertian operator. But the (well-known) difficulty with (2), as far as the quantum theory is concerned,[1] is that it is not compatible with a continuity equation

$$\frac{\partial}{\partial t}(\phi^*\phi) + \nabla\cdot\mathbf{j}(\phi,\phi^*) = 0$$

where $\phi^*\phi$ plays the role of a probability density and $\mathbf{j}(\phi,\phi^*)$ is interpreted as the probability current density. Motivated by the existence of this defect in relativistic wave mechanics, Dirac showed how (2) could be factorised into the first order differential equation[2]

$$\left[\sum_{\mu=1}^{4}\gamma^{\mu}\frac{\partial}{\partial x_\mu} + \left(\frac{mc}{\hbar}\right)\right]\psi = 0 \tag{3}$$

and its conjugate equation. The latter formalism, in turn, does incorporate a continuity equation in terms of the probability density $\psi^{\dagger}\psi$, as required by the postulates of the quantum theory. In this way, we pass from a description in terms of a one-component complex field ϕ, to a complex, four-component spinor field ψ, to describe the electron.

Both equations, (2) and (3), are compatible with Schrödinger's operator formalism and with the invariance requirement of special relativity theory. Yet, (3) incorporates a continuity equation while (2) does not. Further, (3) entails spin degrees of freedom (which are empirically necessitated by the data) and (2) does not. The Dirac equation (3) is then a more general representation of a relativistically invariant theory of quantum mechanics than is the Klein-Gordon equation (2).

To demonstrate the *implicit* dependence of the solutions of the Klein-Gordon equation on the spin degrees of freedom, we note that if we should operate on the Dirac equation (3) with the linear operator $\left(\sum\gamma^{\nu}\frac{\partial}{\partial x_\nu} + \frac{mc}{\hbar}\right)$, we then recover the Klein-Gordon form

$$\left\{\Box + \left(\frac{mc}{\hbar}\right)^2\right\}\psi = 0$$

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[1] This difficulty occurs only in the "first quantized" theory; it does not happen when the fields are assumed to be quantized. However, other difficulties occur in this case. Some of them will be discussed later.

[2] To read this paper, it is really not necessary to have a detailed knowledge of the features of the equations that are presented. It is sufficient for the reader to be aware of the fact that there do exist certain equations which play particular roles in quantum mechanics. For those who may be interested in the derivations of these equations and the definitions of the symbols in them, there are many excellent texts. See, for example, P. A. M. Dirac (1958) and S. S. Schweber (1961).

But since we started from (3) and not (2), we know from the outset that the solutions ψ are four-component spinor variables and not the one-component scalar variable of the Klein-Gordon theory.

To further exploit this example, we note, upon closer examination, that while (3) is indeed a more general mathematical representation of relativistic wave mechanics than is (2), it is not the most general form of the theory. This is because the theory of relativity is based on a principle that entails *motion*—a *continuous* entity. That is, there is no requirement in the theory of relativity that the laws of nature should be invariant with respect to reflections in space or time. But the Dirac equation (3) does have this reflection symmetry. A reformulation of the wave mechanical equations that maintains its form only under the continuous transformations between relatively moving frames of reference then leads to the more general mathematical representation in terms of 2-component spinor variables (η, χ) which solve the simultaneous equations:

$$\begin{aligned} \sum_\mu \sigma^\mu \frac{\partial}{\partial x_\mu} \eta &= -\left(\frac{mc}{\hbar}\right)\chi \\ \sum_\mu \delta^\mu \frac{\partial}{\partial x_\mu} \chi &= -\left(\frac{mc}{\hbar}\right)\eta \end{aligned} \tag{4}$$

The two-component spinor form of the Dirac equation (4) is in fact the most primitive mathematical representation of wave mechanics; it predicts the maximum information about free matter in the microscopic domain.

3 THE SPINOR FIELD equations (4) describe a free particle with mass m and spin angular momentum $\frac{1}{2}\hbar$. To describe the interaction between this particle and other matter we can introduce the *additional physical assumption* that such interaction is reflection symmetric—if this is in agreement with the experimental data. (It is not so in all cases.) When ψ describes an electron, the explicit effect of a background electromagnetic field is introduced by generalising the gradient operator as follows:

$$\frac{\partial}{\partial x_\mu} \rightarrow \frac{\partial}{\partial x_\mu} + ieA_\mu \tag{5}$$

where A_μ is the electromagnetic four-potential. In the limit where v/c is small compared with unity and with the assumption that the source of the electromagnetic potential is stationary, so that $A_4 \neq 0$, $A_j = 0$, $(j = 1,2,3)$ the Dirac equation, with the generalisation (5), then reduces to the Schrödinger form

$$\left(-\frac{\hbar^2}{2m}\nabla^2 + eA_4\right)\psi_S \equiv \hat{H}_S\psi_S = i\hbar\frac{\partial\psi_S}{\partial t} \tag{6}$$

Note that the Schrödinger solution ψ_S must now be viewed as a mathematical approximation for the Dirac solution ψ, which, in turn, is a special (but not the most general) representation of the Dirac two-component spinor solutions (η, χ) of an equation with the structure of (4).

Recall that the solutions of differential equations are not uniquely prescribed until the boundary conditions have been specified. One of the boundary conditions imposed on the wave equation (6) (or (4) or (3)) is based on the interpretation of their solutions in terms of a probability density. It is the requirement that the solutions must be square integrable, i.e.

$$\int \psi_S^* \psi_S \mathrm{d}V = \text{finite number} \tag{7}$$

When this condition is imposed, along with the boundary condition that the solutions must vanish identically along the boundaries of the space that is supposed to contain the particle (with probability unity), it follows that so long as the potential A_4 in the wave equation is time-independent, equation (6) reduces to the eigenfunction equation

$$\hat{\mathrm{H}}_S \psi_S(n) = \lambda_n \psi_S(n) \qquad (n = 1,2 \ldots) \tag{8}$$

The comparison of the eigenfunction equation (8) with the experimental observations of the "stationary states" of atomic systems leads to a correct correlation between the observed quantities of energy transfer between atoms and the numbers $(\lambda_n' - \lambda_{n'})$ that are computed from the wave equation (8). In addition to the energy spectrum, the other properties of atomic systems also have operator equivalents (similar to the Hamiltonian $\hat{\mathrm{H}}$) and there are corresponding eigenfunction equations in the same solutions $\{\psi_S(n)\}$ (or linear combinations of these solutions) that lead to the spectrum of values of these other atomic properties. It was then concluded from the remarkable agreement between the predictions of these eigenfunction equations and the observations of atomic systems that wave mechanics is an accurate description of the microscopic domain.

4 WHAT ARE THE ESSENTIAL mathematical features of the Schrödinger equation (8)? First, it is a linear equation. This means that the Hamiltonian operator $\hat{\mathrm{H}}$ that determines the spectrum of solutions $\{\psi(n)\}$ is independent of the solutions themselves. An important consequence of this feature is that if $\psi(1)$, $\psi(2)$, . . . are different possible solutions of the equation (8), then any linear combination

$$\sum_{n=1}^{\infty} a_n \psi(n),$$

where a_n are arbitrary coefficients, is also a valid solution.

The linear feature of the wave equations (of the forms (8), (4) or (3)) led its discoverers to assert a fundamental principle—*the principle of linear superposition*. But stated as a principle, the claim is then made that not only under the circumstances where the Schrödinger equation (or the Dirac equation) have been successful in describing the data, but under all possible circumstances to be encountered in future experimentation, the differential equations that describe matter, in the most primitive expression, must necessarily be linear, with an eigenfunction form like (8).

A second interesting feature of the quantum mechanical wave equations is that they are invariant in form with respect to a shift of the spatial origin of the coordinate frame that is used. It then follows from this feature that three numbers are time-independent. These are given by the integrals

$$\kappa\int\psi^*\frac{\partial\psi}{\partial x_j}\,\mathrm{d}V \qquad (j=1,2,3) \tag{9}$$

where κ is a constant to be determined from experimental observations. The three time-conserved numbers given in (9) are then identified with the three components of the momentum of a particle. To fit the measured values for the momenta of microscopic matter, the constant κ is found to have the value $-ih/2\pi = -i\hbar$, where h is Planck's constant. Thus, with this linear formalism, we have established a one-to-one correspondence between the time-conserved momentum, p_j, and the linear operator, $-i\hbar\,\partial/\partial x_j$, according to the rule

$$p_j \Leftrightarrow \int\psi^*(-i\hbar\partial/\partial x_j)\psi\,\mathrm{d}V$$

At this stage we note the following relation:

$$-i\hbar\frac{\partial}{\partial x_j}(x_k\psi)-x_k(-i\hbar)\frac{\partial}{\partial x_j}\psi = -i\hbar\delta_{jk}\psi, \qquad \delta_{jk}=\begin{cases}1 & j=k\\ 0 & j\neq k\end{cases}$$

Or, expressed in terms of the commutator operator [,],

$$\hat{p}_jx_k-x_k\hat{p}_j \equiv [\hat{p}_j, x_k] = -i\hbar\delta_{jk} \tag{10}$$

where $\hat{p}_j$ is the linear operator, $-i\hbar\partial/\partial x_j$, that corresponds to the observed linear momentum components according to the Schrödinger prescription (9). It follows from the noncommutation of the components of the momentum operator and the position operator (defined along the same direction) that according to the rules for computing observables from quantum mechanics one cannot simultaneously specify the momentum and position values for an electron in a particular state of motion.

Heisenberg interpreted this result as meaning that one could not measure all dynamical variables of an elementary particle simultaneously to arbitrary accuracy. For it can be shown from (10) that if Δp is the root-mean-square deviation for a measured value of momentum, $[\langle p^2\rangle-\langle p\rangle^2]^{\frac{1}{2}}$, where $\langle p\rangle$ is the average of the measured momentum in some direction, and if Δx is the corresponding root-mean-square deviation in the position measurement (along the same direction), then according to the way in which the average values are defined in this theory (and the fact that the operators are linear and hermitian) it follows that the inequality

$$\Delta p \Delta x \geqslant \hbar/2 \qquad (11)$$

must be true. Thus we see that if Δp is very small—corresponding to a very accurate knowledge of the electron's momentum then Δx would be correspondingly large; it would be of order $\hbar/\Delta p$—corresponding to a very inaccurate knowledge of the electron's position. Similarly, an accurate knowledge of the electron's position would imply a correspondingly inaccurate knowledge of the electron's momentum, of order $\hbar/\Delta x$.

But then, similar to the generalisation that was evoked in the case of the linearity of the fundamental equations, an axiom was asserted here that would accommodate the Heisenberg relations (11) but also extend beyond the model where it was found to be empirically valid. This assertion was called the 'Heisenberg Uncertainty Principle'. The principle is essentially epistemological. It claims that not all of the dynamical variables of a system of elementary particles can be known simultaneously with arbitrarily accurate values. The implication is that the properties of the elementary particle system that is observed are not predetermined—that they are in fact a consequence of the character of the measurement that is performed. In this sense, then, a theory that adopts this principle as an underlying axion is subjective and nondeterministic.

The mathematical expression (11) of the Heisenberg uncertainty principle is indeed in accord with the structure of the Schrödinger equation and the rules that are evoked to compute observables from its solutions. But *as a principle* it asserts that the nondeterministic description of matter that is used in nonrelativistic quantum mechanics is in fact true under all possible circumstances—even those conditions that have not proven to be a good test of the Schrödinger (or the Dirac) formalism! What I have in mind here is the attempt to extend the Schrödinger formulation so as to be entirely consistent with the theory of special relativity. The latter entails the so-called relativistic quantum field theory. When this extension is carried out, it is found that the resulting formalism (which at first sight looks like the Schrödinger equation) does not in fact have any solutions!

5 MATHEMATICIANS have been aware, for many years before the onset of quantum mechanics, that if $k = 2\pi/\lambda$ is the wave vector for one out of a linear superposition of (ordinary) waves, with a root-mean-square spread in the wave packet of Δk, and if Δx is the root-mean-square spread in

values of the position of an amplitude of the wave, then the inequality

$$\Delta k \Delta x \geqslant \tfrac{1}{2} \qquad (12)$$

must be true. This follows from the so-called 'Schwarz inequality'. If one should now multiply both sides of this "classical" inequality by the fundamental constant $\hbar$, then the physical statement of the Heisenberg relation is obtained. However, the feature (12) of a linear superposition of waves does not establish the Heisenberg uncertainty relations as an *a priori* fact of nature—just as the measurement of the sum of the three angles of a triangle drawn out on my desk as being equal to 180° does not establish Euclidean geometry as an *a priori* fact of nature. Further, it has not yet been established that the mathematical assumptions that lead to the Schwarz inequality—which is the same as the mathematical basis of the Schrödinger *formalism* (e.g. the assumption that there is a one-to-one correspondence between observables and linear operators in a Hilbert space) is generally valid under all circumstances.

If it is true that the mathematical structure of nonrelativistic quantum mechanics is a valid general representation of physics, then it must certainly apply to the domain of phenomena where relativity theory must also apply. The latter situations correspond to observations in which the relative speeds between microscopic bits of matter approach the speed of light, or, equivalently, to those cases where the quantity of energy-momentum transfer between atomic constituents is comparable with the rest energy, mc^2, of the interacting matter. From the experimental side, it is certainly possible to increase continuously the energy-momentum of elementary particles to those magnitudes that would necessitate the use of a relativistic formulation—assuming that the principle of relativity is indeed an acceptable assertion. In addition, the earliest data on atomic spectra, whose explanation nonrelativistic quantum mechanics has been claiming as one of its outstanding successes, actually requires a relativistic treatment for a consistent description. This is because the complete description of radiating atoms entails the coupling of a (nonrelativistic) atom to emitted (or absorbed) electro-magnetic radiation. The description of the latter part of the system is necessarily relativistic!

From the theoretical side, no mathematically consistent treatment of a fully relativistic quantum theory has ever been provided. Nor has anyone ever shown in a rigorous fashion that such a theory can even exist. An enormous amount of brilliant effort has been expended over the past forty years or so to construct a mathematically consistent relativistic quantum field theory so as to be able to assert the validity of the quantum postulates in the domain of high energy physics. But there has indeed been little success in this direction. The difficulty, as I have indicated earlier, has to do with the fact that when the Schrödinger formalism is extended into the domain that would make it totally consistent with the theory of special relativity, the resulting equations have no solutions. The reason is that infinite quantities appear in a natural way in the relativistic formulation.[1]

Techniques have been developed for subtracting off these infinite quantities (called 'renormalisation') and with this calculational prescription some numbers have been determined in remarkable agreement with the experimental data. Still, no solutions of the relativistic field equations have been demonstrated nor has the renormalisation programme ever been shown to be a mathematically consistent method of calculation.

If we are to accept the postulates of relativity theory, fully, then nonrelativistic quantum mechanics can only claim to be a mathematical approximation for a relativistic quantum field theory. But if the latter formalism has not yet been shown to exist, it follows that nonrelativistic quantum mechanics cannot yet claim to have established the validity of its underlying postulates. Indeed, it seems to me that all it can claim to be is a good mathematical approximation for some general relativistic theory. But there is no guarantee that the general theory which is sought is based on any of the postulates of the quantum theory. At the present stage, all that is required of the general theory is that in the asymptotic domain of sufficiently small energy-momentum transfer between the components of a material system, it should *approach* the linear eigenfunction structure of nonrelativistic quantum mechanics. Yet, the *actual limit* needn't be contained within the structure of the general theory, even though it may be approached as closely as we please. With this asymptotic feature of the general theory it would then be able to reproduce all of the successful results of quantum mechanics in the low energy region—a feature of the general theory that is dictated by the empirical results.

The salient point here is that if the actual limit of the linear eigenfunction equations of quantum mechanics is not contained in the general theory (no matter how closely it may be approached!) then the features of linear superposition of quantum states and the Heisenberg uncertainty relations cannot claim to be fundamental principles. They can only claim to be faithful to the data under very special circumstances where quantum mechanics is a good mathematical approximation to the general theory. This is analogous, for example, to the transition from the Newtonian theory of gravitation to Einstein's theory of general relativity. The former gravitational theory, while yielding a good mathematical approximation to the latter theory, is *in principle*, logically inconsistent with Einstein's theory. For example, Newton's theory is based on the notion of action-at-a-distance and atomism while Einstein's theory is a continuum field theory with the implication of a finite propagation time for the interactions between matter and matter.

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[1] See, for example, the discussion by P. A. M. Dirac (1958) p. 306, sec. 81.

6 THE IMPLICATION of the preceding argument is that it would be reasonable (though not absolutely necessary) that the field equations of the general relativistic theory should asymptotically approach the quantum mechanical formalism in the low energy domain (where the latter is, at least empirically correct). Thus we should expect the distribution of values of the properties of microscopic matter to *approach* a discrete spectrum of values in the corresponding limit. But the general distribution of values of these properties, that is predicted by the relativistic field theory that we are discussing is in fact *continuous* at the fundamental level. As the low energy limit is approached, the continuous distribution then correspondingly approaches a peaked distribution. However, so long as the limit of the eigenfunction formalism is not actually reached, the predicted distribution of values of the properties of matter remains continuous—though the peaks become as sharp as we please.

This conclusion does not violate any experimental data—since all observations in atomic and nuclear physics (and elementary particle physics) entail a finite width that cannot in principle "go away" in any measurement, even according to the quantum theory. The conventional reason that is evoked for this is the assumption of the Heisenberg uncertainty principle. But the existence of a finite width may also be due to the fact that the underlying general theory is really a continuum theory in which the limit of discreteness does not actually exist in principle. An example of such a continuum theory is the one that I have been studying.[1] Here it is argued that a full exploitation of the principle of relativity implies a *nonlinear* spinor field theory of matter in which the nonlinearity is an essential ingredient. With this nonlinear feature of the underlying field equations, there are no discrete eigenstates contained in the solutions —even though this feature of a quantized theory can be approached arbitrarily closely. The "line widths" that follow from this theory are a consequence of the nonlinearity in the underlying equations.

To sum up, it has been argued that since no demonstrably mathematically consistent relativistic quantum field theory has ever been constructed, nor has such a theory been shown to be able to exist, it is still logically and empirically possible that the general theory of matter that correctly describes the relativistic domain is in fact a nonlinear, deterministic, continuum field theory, as anticipated by Einstein. The existence of such a theory to accurately describe the data of microscopic matter in both low energy and high energy physics, would then rule out the admission of the linear superposition principle and the Heisenberg uncertainty principle as fundamental assertions about the properties of matter. With this approach, these features in the description are only valid as mathematical approximations in the asymptotic region of sufficiently small energy-momentum transfer between the interacting components of a material system.

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[1] A description of my approach, along with a bibliography of the mathematical papers that have been published, is given in Sachs (1964), (1967) and (1968).

The conserved properties of matter, such as momentum and energy, would in the latter general theory depend on certain integrals that involve the derivatives of the Lagrangian that yields the nonlinear field equations. (The exact forms of these integrals follows from the application of Noether's theorem in field theory, as it is usually done.) The point here is that in the latter integrals, one cannot identify a linear operator (such as $\partial/\partial x_j$) with a dynamical variable (such as the momentum components $\hat{p}_j$) although the correspondence does evolve from the formalism in the low energy limit where the nonlinear formalism approaches a linear formalism. Still, in the general formulation, the momentum of the matter that is described is defined. It depends on a nonlinear operator, much more complicated than the ordinary gradient operator. In this case, then, $\hat{p}_j \nrightarrow \partial/\partial x_j$ and the commutation relations (10) are not true. Thus, the Heisenberg uncertainty relations are not generally true—they are only useful as a mathematical approximation in a particular asymptotic limit of the theory. This limit, in fact, corresponds to the situation where there is so little energy-momentum transfer between the interacting matter (say the electron and the cloud chamber) that knowledge about the variables that describe the electron becomes correspondingly unprecise. It is here where one can talk of the Heisenberg relations in terms of probabilistic notions. But this does not mean that the electron is intrinsically probabilistic! For as the energy of the electron is increased toward the high energy region (operationally, by turning up the dials of the control panel of the linear accelerator, for example) then the "particle" dynamical variables, p and x, that Heisenberg refers to, become an inaccurate language. Instead we must then proceed to the continuous field description. But the field description that I have been discussing is indeed a nonlinear, deterministic formalism where probability and randomness do not play any role in the exact underlying theory. It follows then, with this approach, that the Heisenberg uncertainty principle and the principle of linear superposition must be rejected *as fundamental* *assertions*. But all of this is still consistent with a correct description of the observed properties of matter in the microscopic domain!

Whether or not the deterministic, nonlinear field theory that I have referred to will continue to be successful in describing the microscopic domain, its very existence has convinced me that while *quantum mechanics* is important as a mathematical approximation to describe low energy phenomena, the conceptual basis of the *quantum theory* is indeed not necessarily true in any domain of physical phenomena.

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Laws and Instantial Statements

by ALEX BLUM

IN *The Structure of Science* Nagel contends that a deductive explanation of the occurrence of an individual event must contain at least one instantial statement[1] as a premiss (Nagel, 1961, p. 31). I shall defend a version of his contention. More precisely, I shall argue for the truth of:

> (*N*) From a set of laws (expressed without individual constants in a standard notation of first order logic with identity) no instantial statement is deducible.

1 NAGEL DEFENDS (*N*) but his argument rests on an assumption which is clearly false. He writes (p. 32):

> The indispensability of initial conditions[2] for the deductive explanation of individual occurrences is obvious as a point in formal logic. For it is logically impossible to deduce a statement instantial in form from statements that have the form of a universal conditional. (For example, it is impossible to derive an instantial statement of the form 'x is B' from a universal conditional of the form 'For any x, if x is an A then x is a B'.) But obvious though the point may be, it is an important one that is frequently neglected in discussions of scientific procedure.

Clearly, it is possible to deduce a statement instantial in form from statements that have the form of a universal conditional. Two such sets of cases are indicated by the following pair of schemata:

(*A*) 1. $(x)(Fx \supset Gx)$
2. $(x)(Fx \supset \sim Gx)$
Hence, $\sim Fa$

(*B*) 1. $(x)(Fx \supset Gx)$
2. $(y)[(x)(Fx \supset Gx) \supset Hy]$
Hence, Ha

2 THE ARGUMENT which I propose to offer in defence of (*N*) rests chiefly on a version of Nagel's account of contrary-to-fact conditionals, and on the following three postulates:

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[1] I use 'instantial statement' in place of 'singular atomic statement which is not logically true'. By 'atomic statement' I mean 'statement expressible in a sentence which contains just one occurrence of just one predicate letter'. ('=' is counted as a predicate letter).

[2] By 'initial conditions' Nagel (1961, pp. 31–2) here means statements of initial conditions.

(*P*1) If *L* is a law, *L* supports at least one *bona fide* contrary-to-fact conditional;

(*P*2) If *S* entails and is in turn entailed by a set of laws then *S* itself is either a law or a conjunction of laws; and

(*P*3) If '$f \supset (g \& h)$' is a law (where 'f', 'g', and 'h' are predicate letters representing the atomic sentences in which they occur), so are '$f \supset g$' and '$f \supset h$'.

In defence of (*P*1) we shall here only say, that it is commonly held that a law must support at least one contrary-to-fact conditional (Nagel (1961), pp. 51–2). The qualifier '*bona fide*' has been introduced to save the thesis from triviality. More will be said on this in section 4.

(*P*2) satisfies fully, it would appear, the criteria generally considered relevant in classifying statements as laws of nature. It is generally held that: (*a*) laws must be universal in form; (*b*) some of the available evidence for a law must be indirect (in at least one of the senses of 'indirect' to be given below); and, more vaguely, (*c*) a law must be deductively related in some ways to some of the other laws in a scientific system (Nagel (1961), Ch. 2, especially p. 68).[1]

(*a*) may be understood in either of two ways. (*a*) may be understood as the requirement that a law must be expressible as a universal conditional. So understood, (*a*) is trivially satisfied by any statement and hence by *S*. (*a*) however may also be understood as the requirement that a law must be expressible as an "irreducible universal conditional". That is, as a universal conditional which is not logically equivalent to a non-universal statement. But any consistent statement which entails an irreducible universal must itself be an irreducible universal. Hence, if laws must be expressible as irreducible universals, *S* is also an irreducible universal.

The satisfaction of (*b*) appears to be assured on both of the usual accounts given of "indirect evidence". It is usually held that (i) if a law *L* is jointly derivable with a set of other laws from a more general law, say *K*, then the direct evidence for the set of jointly derivable laws is indirect evidence for *L*; and that (ii) if a law *L* in conjunction with a set of special assumptions entails a set of laws each with a distinctive scope of predication then the direct evidence for the set of entailed law is indirect evidence for *L*.

Let L_1 be a law (or a conjunction of laws)[2] which entails *S* and let L_2

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[1] Criterion (*c*) is meant to include conditions (2) and (3) in Nagel (p. 68).

[2] The above accounts of "indirect evidence" are limited to laws. However, our extrapolation to "a conjunction of laws" cannot be too harmful in view of the point of (*b*) (Nagel, pp. 67–8), the fact that a conjunction of laws is expressible as a universal conditional and the fact that L_1 is deducible from L_1. Furthermore, and perhaps more importantly, since it is sufficient for *S* to satisfy either (i) or (ii) in order for (*P*2) to satisfy (*b*), nothing really hinges on the extrapolation to "conjunction of laws".

be a law which is entailed by S. Clearly if L_1 is jointly derivable with a set of other laws from a more general law, then so is S. Now, if S provides indirect evidence for L_1, then S is a law. And, if S does not provide indirect evidence for L_1, then whatever indirect evidence is provided for L_1 is also provided for S. Hence on (i) ($P2$) satisfies (*b*).

Again, whatever laws are entailed by L_2 in conjunction with a special set of assumptions, are also entailed by S in conjunction with the same set of assumptions. Hence, whatever indirect evidence is available on (ii) for L_2 is also available for S.

The satisfaction of (*c*) appears to be assured by the fact that S both entails and is entailed by a set of laws.

($P3$) embodies the recommendation that unless both '$f \supset g$' and '$f \supset h$' fulfil the requisite requirement of lawness their conjunction, i.e. '$f \supset (g \& h)$' should not be considered a law. ($P3$), I believe, guarantees that if '$f \supset (g \& h)$' fulfils the above conditions (*a*) to (*c*) in letter, it will also fulfil them in spirit. ($P3$) prevents either 'g' or 'h' from being an idle partner.

3 FROM *The Structure of Science* (pp. 70–3) it would appear that Nagel holds:

> (*Na* 1) A counterfactual 'p *CF* q' (and its subjunctive correlate, the subjunctive conditional 'p *SU* q')[1] is supported by a law L if and only if '$L, p \vdash q$' is true.[2] A counterfactual 'p *CF* q' (and its subjunctive correlate, the subjunctive conditional 'p *SU* q') is supported if and only if there exists a set of laws $\{I\}$, so that '$p, I_1, I_2, I_3, \ldots, I_n \vdash q$' is true (where $I_1, I_2, I_3, \ldots, I_n$ are elements of $\{I\}$).
>
> (*Na* 2) A subjunctive conditional 'p *SU* q' is true if and only if 'p *SU* q' is supported.[3]
>
> (*Na* 3) 'p *CF* q' is logically equivalent to '(p *SU* q) & $\sim p$ & $\sim q$'.
>
> (*Na* 4) 'p *SU* q' and 'p *SU* $\sim q$' are contraries.
>
> (*Na* 5) 'p *CF* q' and 'p *CF* $\sim q$' are contraries.

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[1] '*CF*' is a counterfactual statement connective, connecting the antecedent and the consequent clauses of the counterfactual. '*SU*' is a subjunctive sentential connective, connecting the antecedent and consequent clauses of a subjunctive conditional. Throughout this paper we shall assume that the expressions flanking '*SU*' like the expressions flanking '*CF*' are either statements or statement-schemata.

[2] In order for this statement to be true, we shall assume that the necessary statements of "the requisite initial conditions for the law" (Nagel, p. 72) are entailed by the antecedents of the counterfactuals and subjunctive conditionals under consideration.

[3] Nagel's claim that the accidental universal 'all the screws in Smith's current car are rusty' supports the subjunctive conditional 'if any screw were *identical with* one of the screws in Smith's car it would be rusty' (p. 52, note 4) apart from being incompatible with the rest of his treatment is surely false. For, if a screw in Smith's car were *identical* with a screw that is not rusty, then not all the screws in Smith's car would be rusty.

Should (Na 1) to (Na 5) be accepted, it would immediately follow that:

($A1$) $(x)(Fx \supset Gx)$

and,

($A2$) $(x)(Fx \supset {\sim}Gx)$

cannot both be laws[1] and hence (A) is not a genuine counter-example to (N). For by (Na 1) ($A1$) and ($A2$) support (respectively) both '$Fa\ SU\ Ga$' and '$Fa\ SU \sim Ga$' and thus by (Na 2) they are both true. But this is clearly inconsistent with (Na 4). Hence ($A1$) and ($A2$) cannot both be laws.

(Na 1) to (Na 5) however is unacceptable. For should (Na 1) to (Na 5) be accepted, it would immediately follow that there are *no* laws. For if S is a law and p is inconsistent with S then (by (Na 1) and (Na 2)) both '$p\ SU\ q$' and '$p\ SU \sim q$' are true. But this, again, is clearly inconsistent with (Na 4).

We can avoid this unacceptable result while at the same time we preserve the sound insights expressed by (Na 1) to (Na 5) by the following reformulation:

(Na 1a) A counterfactual '$p\ CF\ q$' (and its subjunctive correlate, the subjunctive conditional '$p\ SU\ q$') is supported by a law L if and only if '$L, p \vdash q$' is true. A counterfactual '$p\ CF\ q$' (and its subjunctive correlate, the subjunctive conditional '$p\ SU\ q$') is supported in a world W if and only if the law(s) of W[2] conjoined with p, entail q.

(Na 2a) A subjunctive conditional '$p\ SU\ q$' is true of a world W if and only if '$p\ SU\ q$' is supported by the law(s) of W.

(Na 3a) ''$p\ CF\ q$' is true in W' is logically equivalent to ''$p\ SU\ q$ & $\sim p$ & $\sim q$' is true in W'.

(Na 4a) '$p\ SU\ q$' and '$p\ SU \sim q$' are contraries in W if and only if 'p' is possible in W (i.e. 'p' is consistent with the laws of W).

4 (Na 1a) TO (Na 4a) reflect, among others, the following observations.

If someone asserts a subjunctive conditional whose consequent (though logically consistent) is inconsistent with our laws of nature, then (normally) his subjunctive conditional is being asserted not about our world but about some other possible world; one whose laws are consistent with the antecedent of his conditional.[3] Clearly, should someone assert a conditional beginning with 'If this ship were to travel at a velocity greater than that of light...' we would normally say that his conditional is not about our world, but about a world whose laws do not entail the theses of the special theory of relativity. To know which subjunctive conditonals are true of this world we must know its laws.[1]

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[1] ($A1$) and ($A2$), strictly speaking, are schemata. Where there is little chance of confusion we shall speak of sentence-schemata as though they are sentences.

[2] The defining characteristics of a world W are, for us, its laws of nature.

[3] We here assume that normally one does not think that a set of statements is consistent when in fact it is not, and that there is no disagreement as to which statements are laws. However, nothing really hinges on these assumptions except ease of exposition.

It is not, perhaps, so obvious, but I think it is equally true that if someone asserts a subjunctive conditional whose consequent (though not logically true) is entailed by the laws of nature, then (normally) his subjunctive conditional is being asserted not about our world but about some other possible world; one whose laws do not entail the consequent of his conditional.

If someone, however, were to insist that although the antecedent of his subjunctive conditional is incompatible with the laws of nature his conditional is still about *our* world (and not some other possible world) we would upon reflection have little hesitation in classifying it as true. For if the antecedent of his conditional were in fact true, the consequent of his conditional would also be true. The same, perhaps even more clearly, holds true for subjunctive conditionals whose consequents are entailed by the laws of nature of the world they are about. That is, if someone were to insist that although the consequent of his subjunctive conditional is entailed by the laws of nature, his conditional is still about our world, we would upon reflection have little hesitation in classifying it as true. For if the consequent of his conditional is true of our world unconditionally, then it is surely true conditionally. Should someone assert 'If this ship were green it would travel at a velocity less than that of light', we would undoubtedly be taken aback, for it is trivial. But we would surely classify it as true.

Our discussion suggests a threefold classification for subjunctive conditionals: *trivial*, *insignificant*, and *significant*.

A subjunctive conditional is *trivial*, if and only if: (*a*) it is true of every possible world; or (*b*) its consequent is unconditionally true of the world it is about; i.e. its consequent is entailed by the laws of nature of the world it is about.

A subjunctive conditional is *insignificant*, if and only if its antecedent could not possibly be true of the world it is about, i.e. its antecedent is inconsistent with the laws of nature of the world it is about.

A subjunctive conditional which is neither *trivial* nor *insignificant*, is *significant*.

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[1] That is if we want to know all the subjunctive conditionals which are true of this world. This of course is not to say that we cannot know whether or not any subjunctive conditional is true of a world without knowing all its laws. Should we know nothing of a world except that one of its laws is Newton's law of universal gravitation we would immediately know an infinite set of logically independent subjunctive conditionals which are true of that world.

We shall classify counterfactuals with their correlate subjunctive conditionals. We shall say that a counterfactual is ***trivial***, ***insignificant***, or ***significant***, if and only if, its correlating subjunctive conditional is (respectively) ***trivial***, ***insignificant***, or ***significant***.

By requiring that if L is a law, L must support at least one ***bona fide*** counterfactual (i.e. $(P1)$), we require that there be at least one counterfactual supported by L which is both true and ***significant***. This amounts to requiring that if L is a law, L must support at least one true counterfactual which is in need of support (i.e. one whose correlative subjunctive conditional is not logically true) and which is about a possible state-of-affairs (i.e. one whose antecedent is possible in the world it is about; that is, one whose antecedent is consistent with the laws of nature of the world it is about).

5 FROM WHAT we have so far said it immediately follows that argument schemata (A) and (B) are not genuine counter-examples to (N).

We shall first deal with (A).

The set of subjunctive conditionals which are correlates of the set of counterfactuals supported by

$(A1)\quad (x)(Fx \supset Gx)$

are logically equivalent to instances of one of the following three schemata[1]

$(A1\text{a})\quad K\ SU\ (Gc \vee M)$

where K entails '$(x)Fx$', 'G' and 'c' are constants and M is any statement whatsoever.

$(A1\text{b})\quad K'\ SU\ (Gc \vee M)$

where K' entails 'Fc'; or,

$(A1\text{c})\quad K''\ SU\ ((\exists x)Gx \vee M)$

where K'' entails '$(\exists x)Fx$'. And the set of subjunctive conditionals which are correlates of the set of counterfactuals supported by

$(A2)\quad (x)(Fx \supset \sim Gx)$

are logically equivalent to instances of one of the following three schemata:

$(A2\text{a})\quad K\ SU\ (\sim Gc \vee M)$

$(A2\text{b})\quad K'\ SU\ (\sim Gc \vee M)$

or

$(A2\text{c})\quad K''\ SU\ ((\exists x) \sim Gx \vee M)$

But $(A1)$ conjoined with $(A2)$ entails '$(x) \sim Fx$'. Hence, if $(A1)$ and $(A2)$ are both laws, then since K, K', and K'' are all inconsistent with '$(x) \sim Fx$', it follows that neither supports a ***bona fide*** counterfactual.[2]

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[1] We are considering only those counterfactuals and the subjunctive correlates of only those counterfactuals which are not obviously non-*bona fide.*

[2] As may be verified, non-significance is invariant with respect to logically equivalent subjunctive conditionals containing an atomic expression which is either inconsistent with or is entailed by the laws of nature.

Thus by ($P1$), ($A1$) and ($A2$) cannot both be laws.

We now turn to argument schema (B).

If

($B1$) $(x)(Fx \supset Gx)$

and

($B2$) $(y)[(x)(Fx \supset Gx) \supset Hy]$

were both laws, by ($P2$)

($B3$) $(y)Hy$

would also be a law. But the set of subjunctive conditionals that ($B3$) supports (if ($B3$) is a law) is logically equivalent to instances of

($B3$a) *R SU T*

where R is any statement and T is entailed by '$(y)Hy$'.[1] That is, there is no *significant* subjunctive conditional supported by ($B3$). Hence, ($B3$) does not support any *bona fide* counterfactuals and thus it is not a law. It therefore follows (by ($P2$)) that ($B1$) and ($B2$) cannot both be laws.

Interestingly enough, it also follows that laws must be expressible as genuine conditionals, i.e. as conditionals not reducible to atomic statements. For clearly, the failure of ($B3$) to support a *bona fide* counterfactual is a direct result of its logical form.

6 We have shown that neither argument schema (A) nor argument schema (B) is a legitimate counter-example to (N). We shall now show that there are no counter-examples to (N).

Let us assume that there is an instantial statement, say 'Fbc', which is deducible from a set of laws (C). It would then follow that:

(1) '$(x)(y)Fxy$' is deducible from (C)[2]

and

(2) either

(a) $\ldots (x)(y) [\ldots \supset Fxy]$

or

(b) $\ldots (x)(y) \ldots [\ldots \supset Fxy \ldots]$

is in (C) and hence by ($P3$), (a) is a law.[3]

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[1] See p. 375.

[2] Since none of the elements of (C) contains individual constants, the choice of constants introduced in the derived steps is purely arbitrary. That is, if the universe of discourse consists of elements denoted by 'a', 'b', 'c', . . ., then if a step, say 'Ga', in which the individual constant 'a' is introduced, is deducible form (C), so are 'Gb', Gc', . . ., and thus so is '$(x)Gx$'. Consequently, if any step containing an individual constant is valid, so is the step in which all occurrences of that individual constant are suitably replaced by universally quantified variables.

[3] That either (a) or (b) must be in (C) follows from the following three considerations:

(i) A set of rules for natural deduction is complete if it contains:

(ia) No detachment rule other than *Modus Ponens*, and R: $p \supset (q \& r) / \therefore p \supset q$; and

(ib) A standard set of non-detachment rules (e.g. Copi's rules (Copi (1967), pp. 36, 42, 85, 108–14, 161) except for *M.T.*, *D.S.*, *C.D.*, *D.D.*, and *Simp.*).

The proof of (i): Copi's rules are complete (Copi (1967), pp. 247, 323) and the only rule

But by ($P2$) if (a) is a law (or conjunction of laws), so is '$(x)(y)Fxy$'. '$(x)(y)Fxy$' however cannot support any *bona fide* counterfactuals and thus by ($P1$) it is not a law (or a conjunction of laws).[1] Hence, no instantial statement is deducible from a set of laws (when these laws are expressed without individual constants in a standard notation of first order logic with identity).

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excluded by (ia) and (ib) which is not clearly redundant is *Simplification.* For this we have R.

1. $a\&b$	*assumption*
2. $b\&a$	1, *Comm*
3. $(\sim c \vee c) \supset (a\&b)$	1, *Add, Comm, Impl*
4. $(\sim c \vee c) \supset a$	3, R
5. $(\sim c \vee c) \supset (b\&a)$	2, *Add, Comm, Impl*
6. $(\sim c \vee c) \supset b$	5, R
7. c	*assumption of limited scope*
8. $c \vee c$	7, *Taut*
9. c	8, *Taut*
10. $c \supset c$	7-9, *C.P.*
11. $\sim c \vee c$	10, *Impl*
12. a	4, 11, *M.P.*
13. b	6, 11, *M.P.*

(ii$A$) We shall say that a sentence p is in normal conditional form iff
  (iiAa) p is in prenex normal form
  (iiAb) The consequent of p contains no sentential connective other than negation and conjunction and no negation over conjunction.

(ii$B$) Any statement may be expressed in normal conditional form. [(ii$B$) follows from the fact that any statement may be expressed in prenex normal form and its matrix in (not Boolean) disjunctive normal form, and thus by *association* and *implication* in conditional form.]

(iii) (C) is a consistent set.

If 'F..' (or any other atomic sentence which is not logically true) is deducible from a set of consistent sentences S, then (given (ii$B$) (i) and (ia)) there exists at least one sentence in S expressible as a normal conditional that has 'F..' in its consequent. For if no sentence in S is expressible as a normal conditional with 'F..' in its consequent, then (given (i) and (ia)) 'F..' would not be detachable from S and hence for the purpose at hand (i.e. that of deducing 'F..' from S) it would be as though 'F..' had no occurrence in S at all. But then by considering the argument used in the previous note, not only would 'F..' be deducible from S but so would '$\sim F$..'. Or, to put it differently, if no sentence in S is expressible as a normal conditional with 'F..' in its consequent, then (given (i) and (ia)) 'F..' would have to be introduced (by addition) in a step which is deducible from S (but which entails no element of S.) But if an introduced expression is deducible from S then any expression is deducible from S. For any expression may be introduced. Hence, if no sentence in S is expressible as a normal conditional that has 'F..' in its consequent, then either 'F..' is not deducible or both 'F..' and '$\sim F$..' are deducible.

Hence, given (iii), (a) or (b) must be in (C).

[1] Clearly, on our account of counterfactuals, if L supports a *bona fide* counterfactual so does $L\&S$ where S is any statement whatsoever.

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Discussion

NEW LIGHT ON BRAIN PHYSIOLOGY AND FREE WILL?

PHILOSOPHERS have fretted periodically whether scientific discoveries about our brain will demonstrate that we never act freely. Professor MacKay (1967), as a physicist doing research on information theory and the human brain has, not surprisingly, original views on this enigma. His theme is how, instead of disproving our freedom, mechanistic causal accounts of our brain processes actually guarantee it. On MacKay's interpretation, some of the cerebral events in your future that correspond to your decisions cannot, from *your* "standpoint", be causally determined or even foreknown. In saying these uncaused and unpredictable events constitute free decisions, MacKay evidently assumes that determinism and foreknowledge are incompatible with freedom (p. 17). Before presenting *a priori* objections, however, we should see whether brain physiology can support MacKay's assumption.

Although MacKay and others have written previously on this topic, his 1967 lecture deserves separate attention because it develops his earlier ideas more fully. It also avoids the most overworked arguments in this area. For example, he ignores the deliberate falsification gambit: In principle you can always act or decide differently, if you want to spoil my prediction. Another worn-out argument only crops up briefly. MacKay's version goes (p. 18):

> Regardless of any brain theory, if a prediction specifies that you will take the decision at (*and not before*) 7 p.m. to go for your train, then clearly you must be wrong to believe it *as certain and inevitable* beforehand, since believing it as certain and inevitable would amount to deciding the outcome *before* 7 p.m.—which would refute the prediction! . . .
>
> At 7 p.m., on the other hand, you are ready to decide. It is no longer self-contradictory for you to accept G [the belief that you will decide to go] or W [the belief that you will decide to wait].

This passage encapsulates the familiar and much criticised notion that there is a *logical* tie between believing that you will decide to do *X*, and deciding already to do *X*. MacKay's 1967 lecture is novel because its central argument clashes with this "deciding in advance" doctrine. He contends repeatedly that brain physiology indicates a *causal* connection between your beliefs and your decisions (pp. 8–9, 11–15, 19–20).

More precisely, MacKay's causal hypothesis is that cerebral processes which "represent" your present beliefs are among the factors that will bring about cerebral processes representing your future decision. By 'represent' MacKay means that these neural events are "correlated" with, or correspond to, your mental activities (pp. 14–15, 25 n.). MacKay does not exclude the possibility that brain processes are contingently identical with the mental goings-on they represent. But he rightly denies that a neurophysiological description of a cerebral process is synonymous with your report, in ordinary mentalistic language, of your believing such-and-such, although your believing might be the same event as the cerebral process (pp. 28–9). For simplicity, I take contingent identity of brain processes and mental activities for granted.

MacKay's conclusions sound highly paradoxical. The message he extracts from brain theory is that from the "standpoint" of an observer, all your future decisions are caused and predictable. From your standpoint *after* you have made up your mind, the same holds. But before you reach your decision, brain theory shows that it is causally impossible for you to predict what you will decide. *Ergo*, from your standpoint, your future decision has no cause (pp. 22–9). I will not criticise MacKay's discussion of 'standpoint'; I cannot reformulate what he says with sufficient brevity and clarity. But even if that much of his argument is taken on trust, enough errors remain in MacKay's reasoning to justify rejecting his conclusions.

I will make a comment about MacKay's two "standpoints", however. The claim I accepted above, that neurophysiological and mentalistic ordinary language descriptions differ in meaning, has no bearing on MacKay's conclusion regarding standpoints. MacKay argues fallaciously from this claim (pp. 28–9):

> When the Determinist produces his story about your brain, and you produce yours about your decision, it would be a philosophical blunder to imagine that his story could be a *translation* of yours. Indeed . . . this is so far from being the case that it is possible for his story to be deterministic where yours is indeterministic, without mutual contradiction (indeed with mutual corroboration). Although they both relate in a sense to the same events, they are not making assertions about the same aspects of them.

MacKay's first mistake here is assuming that observers must talk neurophysiological jargon. The observer knows what future decision is represented by the brain process he forecasts. So he can assert simply: 'You are going to decide to do *X*'. Then the only "aspect" at issue between you and the observer is whether any events from now until then will cause that decision. The fact that neurophysiological and mentalistic reports differ in meaning cannot settle that issue. A second mistake emerges here as well. How can MacKay imagine that the deterministic "story" of the observer and your indeterministic account are both correct answers to the question: 'Will anything cause your future decision?' If one story is that your decision will be caused, and the other is that nothing will cause your decision, then both stories concern the same aspect of the event which is your future decision, and conflict with each other.

The fallacy I have been exposing is a straightforward *non sequitur*, where an acceptable premise about language has no bearing upon MacKay's conclusion that determinism is true from the standpoint of an observer, but not for the agent. The next defective inference that concerns me is more complex. Here two crucial ambiguities in MacKay's mechanistic causal hypothesis make it sound as if this hypothesis entails that we cannot predict what you will decide to do. Why not? He wants to prove that this is due to the incalculable effect of your believing, disbelieving, or (a possibility he should add) suspending judgment upon any prediction we make. For this he offers very general evidence that many of our cognitive attitudes have some unspecified effect upon our decisions. In neurophysiological terms, the processes that occur in our brains when we believe, disbelieve and suspend judgment on anything influence the processes that will occur in our brains when we settle upon a course of action. MacKay says (p. 9) that Determinists have overlooked

> a small factor which in almost every other context can safely be neglected: namely, the change that must take place in the physical brain (according to mechanistic brain theory itself) when knowledge is acquired by a human agent . . .
>
> . . . If a proposition happens to describe, implicitly or explicitly, the brain-state which reflects what you believe, its accuracy may depend upon whether or not you believe it.

To make matters worse, his illustration here is not even an example of belief about decision affecting decision. It is a case of your cognitive attitude toward a "proposition" which describes your beliefs affecting (the brain state correlated with) your beliefs! MacKay gives a more relevant, but equally general illustration when he argues (p. 19):

> Your choice . . . is genuinely undetermined: an event which depends on your cognitive attitude, as distinct from the myriad other bodily happenings (for example in your digestive system) which are unaffected by your attitude to them . . .

Apart from the contradiction in describing your choice both as "undetermined" and as dependent upon your cognitive attitude, what is wrong here? First, this and the previous example barely support MacKay's hypothesis that a *cause* of changes in your future decision is your belief, disbelief or indifference toward the prediction *that you will decide to do X*. Secondly, there is no warrant for concluding that, as a result of your cognitive attitude toward the prophesy that you will decide to do *X*, it is likely that you will decide to do *Y*. As I remarked earlier, MacKay's concern with mechanistic brain theory makes the deliberate falsification problem irrelevant.

To the question at issue, 'Does your cognitive attitude toward my forecast of your decision alter your decision?', here is MacKay's fullest answer (pp. 11–12):

> Even if the brain were as mechanical as clockwork, no completely detailed present or future description of a man's brain can be equally accurate whether the man believes it or not. (a) It may be accurate *before* he believes it, and then it would automatically be rendered out of date by the brain-changes produced by his believing it; or (b) it might be possible to arrange that the brain-changes produced by his believing it would bring his brain into the state it describes, in which case it must be inaccurate *unless* he believes it, so he would not be in error to *disbelieve* it.

The only unpredictable changes MacKay anticipates, then, are details within the brain process that is your decision. If you had not come to believe, disbelieve or suspend judgment about my forecast, the process in your brain would have lacked those particulars. This is a far cry from MacKay's conclusion, namely that there is no telling whether you will decide to do *X* or *Y*.

Even with regard to *details* of the process which will occur in your brain after you hear my prediction, why does MacKay abandon hope of forecasting them? Suppose we have MacKay's situation (a). Then we prophesy: 'As long as it is *not* the case that you expect brain process *P* (or the decision that *P* represents), *P* will occur; but if you expect *P* (or the decision), then *P′* (whose details differ from those of P) will occur instead'. In situation (b) we predict: 'Process Q will occur if, and only if, you expect Q (or the decision Q represents)'.

I haven't finished with MacKay's denial that we can foretell *minutiae* of your brain process which result from your cognitive attitude toward our prediction. I am convinced that MacKay's equivocations on this premise account for the specious plausibility of his arguments from mechanistic brain theory to his version of indeterminism. His arguments may be paraphrased as follows:

(1) Your cognitive response toward our prediction of your brain process might affect details of your future brain process;
(2) Consequently we cannot give a *complete, accurate* and *final* description of your future brain process;
(3) Our predictions must be *indefinite* on these matters of detail;
(4) It is *indeterminate* for us exactly what process will occur in your brain;
(5) In this sense, we as forecasters cannot *determine*, or know fully, what process will occur;
(6) In other words, it is *undetermined* precisely what will happen in your brain;
(7) The brain process itself is *indeterminate*;
(8) The brain itself is *undetermined*;
(9) The brain process is *not determined*, i.e. not caused;
(10) Therefore, with respect to your future brain processes, the Deterministic Principle, that every event is caused, stands refuted.

Every italicised word is taken from passages where MacKay prepares for or presents his master argument from brain theory to indeterminism (pp. 5, 9–13, 16–20, 24, 26, 28–9, 37). Downright fallacies only begin with step (7). Aside from the reservations I voiced above, premise (1) about forecasting details seems to me quite innocuous. And *if* (1) is true, so are (2), (3), (4), (5) and (6). Nevertheless (2) through (6) are to blame for the invalid inferences that follow.

What fallacious inference occurs? An illegitimate shift from talking about features of our *knowledge* and our *prediction* of your future brain process, to talking, at (7), as if your future brain process had these same features! From the supposition that our knowledge or prediction is indeterminate, it hardly follows that the brain process we foresee is indeterminate. And then, from (7) we go on to hear (8), that the brain process is "undetermined". Are (7) and (8) intelligible? I doubt it. But even if they are genuine assertions these assertions clearly do not follow from (1) through (6).

Finally, suppose we accept (7) and (8). We must still object to (9). No sense in which our incomplete foreknowledge of your brain process makes the process somehow "indeterminate" (7) or "undetermined" (8), can entail that nothing will *cause* the process to occur (9). The previous statements (1) through (8) give no warrant for this outrageous inference. Consequently MacKay's indeterministic conclusion (10), although it does follow logically from (9), has to be rejected along with (9).

Rather than inquire more deeply about what would prove limited indeterminism, I want to ask why MacKay might have thought that epistemological premises on the order of (2) through (6) count against determinism, and demonstrate free will. Does he confuse the epistemological statement that someone *knows* what you will decide or do, with the statement that you *cannot avoid* deciding or acting thus, which concerns your powers as an agent? In the following passage MacKay begins sensibly:

> Some of your most responsible actions may be highly predictable by people who know you well, without any examination of your brain. Continuity in the mental processes leading up to a decision is not merely permissible but positively desirable if you are to be regarded as responsible. Ideally, indeed, most of us like to be able to confirm our responsible decisions by going over the ground more than once, to see whether we come to the same conclusion.

Then he seems to confuse the circumstance that your action is foreseen, with quite distinct possibility that you are forced to perform it, or in some other way unable to act differently. MacKay goes on:

> If, however, before you made up your mind, there existed a determinate and unique specification of the outcome which could command the assent of everyone, including yourself, whether you knew it or not or liked it or not, then the case would be different. Here we would have to say that the outcome was not only predictable by others, but also *inevitable by you*, and you could not be held to have acted 'freely' (p. 17, his italics; see pp. 7–9. 16, 20–1, 31–3, 37).

Further evidence of this confusion appears whenever Mackay writes of "take it or leave it" predictions (pp. 12, 19, 21) and predictions that are "binding" upon the agent (pp. 20, 21; see 31–2). If you are starving and someone offers you a dead rat, this is a "take it or leave it" situation; you have no choice but to take the rat. A promise or contract is binding; by its terms you cannot refuse to perform some action. But the *prediction*, 'You will decide to eat this rodent', does not compel you to decide, or to consume the rat. Nor does the *forecast* that you will fulfil your promise or contract prevent you from acting otherwise.

I conclude that MacKay's mechanistic hypothesis, about how your cognitive response to predictions of your future brain processes might affect those processes, is none too plausible; that it does not count against determinism; and moreover that his whole enterprise is founded upon assimilation of causes and foreknowledge to coercion. These criticisms suggest that there is no problem here on which new light can be shed.

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It is easier to write about Wittgenstein than to write about philosophy. That is not to say that it should be; it is just that in the shadow of the Master it is so much easier to pass off anything sufficiently *puzzled* as the product of hard serious thought. And God is merciful, the supply of manuscripts is plentiful, so we need have no fear that those who come to adore should ever be turned away, without first being taught the essential rite of clutching the brow with one hand and turning the handle with the other.

"Professor van Peursen," says the blurb, "has written a study of Wittgenstein of the kind that is needed . . ." No reader of this *Journal* could possibly need it. Nor could it properly be recommended to students. It is, I suppose, partly a matter of opinion and approach to state at what point a beginner in philosophy should be brought to read the works of Wittgenstein, but I should have hoped that it was beyond dispute that that point could only be long after he had reached a level of understanding far beyond anything this will appeal to, or produce. What we have is a gallop through the earlier and the later Wittgenstein (Thinking Aloud; Wittgenstein's Life and Philosophy; The Logic of Speech; The Metaphysics of Silence; Language Games; Forms of Life) with the later Wittgenstein principally distinguished by a banner labelled 'Use'. There is absolutely no indication that unless parameters of use are given this concept is just monumentally boring—not, however, sterile, for it is an old truth that the sleep of reason breeds monsters. The concept is boring because it is too blunt to say anything about meaning, just as, when you remove the misleading implication that discourse is an end in itself rather than a way of stating, predicting, or analysing the facts, nothing interesting comes from the notion of a language game.

Professor Winch's collection appeals to an entirely different level of understanding. In it we have two essays on the *Tractatus*, one on Wittgenstein on mathematics, three on problems associated with other minds, and one on Wittgenstein on psychoanalysis. The collection is therefore nothing if not heterogeneous. The three papers on other minds and pain may be taken together. Cook's paper ('Human Beings') sets the tone: we have the language-game of human beings, and any argument to justify it can have "no relevance for us in our adult years". And as far as learning it goes, well, we do not saddle the child with the philosophical notion of "body", so, presumably, no proper problem ever arises of justifying attributions of pain, intelligence and so on on the basis of "bodily" or behavioural attributes. I suppose the same argument would do away with any philosophical problem of justification. 'When was I in need of the argument from analogy?' asks Cook, with a view to showing never, because only insane adults and no children start by seeing people as things. Probably only insane adults and no children see the expectation that an unsupported stone will drop, or an object continue to exist when unperceived as problematic: none undergoes a mental process of inferring it from their other experience, yet it is that which justifies the belief. Reinhardt ('Wittgenstein and Strawson on Other Minds') also emphasizes the primacy of our human vocabulary of action and feeling. Together with our responses and attitudes it constitutes a form of life. It's not a case of justifying any attributions via criteria, we don't *get from* one available mode of description to another. Again, what we *get from* to what (this presumably means, what inferences we make) does not determine what must be justified in terms of what. Manser ('Pain and Private Language') argues that Robinson Crusoe could not have had a language: actual publicity is a necessary condition of language. The argument, if good, would show that you can't have a private name for a thing either, which ought to give us pause. Pain behaviour is a form of life. It seems to me that none of these papers adds in the least to our understanding of their subject matter, and I am absolutely certain that each falls far below any faintly acceptable standard of philosophical argument or rigour.

Perhaps Cioffi's paper ('Wittgenstein's Freud') is to be judged only as a rather delightful *jeu d'esprit.* Wittgenstein apparently found Freud's explanations of this and that "charming", "compulsive", and was strongly opposed to the idea that they gave a causal explanation of anything. They gave interpretations, like aesthetic explanations. But it is difficult to see what is so interesting about that as it stands, and the elucidation of what Wittgenstein meant by 'aesthetic explanations' did not help me. I cannot see, for example, what this view of Freud has to do with the discussion of the unfalsifiability of Freudian theory, unless the view really is supposed to reduce Freud's achievement to the level of the "anonymous geniuses to whom it first occurred that Tuesday is lean . . ." Pointing out aspects of things is not, I suppose, inventing them. Shwayder's paper gives a useful list of references to Wittgenstein's work on mathematics, but the philosophical aspect is again weak; why, for example, should the view that mathematics is a "kit of instruments for conceptual control" be an alternative to Platonism?

The first two papers in the book are concerned with the *Tractatus.* Rhees ('Ontology and Identity in the *Tractatus*') takes issue with Max Black, but I found it impossible to locatethe issue. Miss Ishiguro's paper ('Use and Reference of Names') is clearly the most interesting in the collection. Rightly seeing that nothing is expressed by contrasting a "name" theory with a "use" theory, because names must be used in order to name, she goes on to give a careful and in many respects persuasive theory of the names in the *Tractatus*, according to which these are "dummy names" rather than names of objects. The difficulty lies in understanding dummy names. In spite of the clarity of much of the discussion I failed to grasp why, if Miss Ishiguro's analogies with numerals and the ascription of "dummy names" in geometry were correct, Wittgenstein could possibly have thought that atomic propositions, in which such things occur, could be made into an analysis, in any sense at all, of anything which we ordinarily say.

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FANN, K. T., Ed. (1969) *Symposium on J. L. Austin.* London: Routledge. £4.00 (80*s.*) Pp. xii+486.

This collection consists of 26 essays, including some original contributions. The book is divided into four parts, of which the first deals chiefly with Austin's method, and the remaining three with, respectively, the topics of *Philosophical Papers, Sense and Sensibilia* and *How to do things with Words.* There is also an extensive, but not exhaustive, bibliography.

It is apparent from the size of the collection that it is not intended to be merely an introduction to Austin's views, and the impression gained is that the editor's aim is to present, through his choice of essays, an *orthodox* account of Austinian philosophy—an impression confirmed by the presence of a frontispiece photograph of Austin. Orthodoxy is represented by J. O. Urmson and G. J. Warnock in the "introductory" first part, and chiefly by L. W. Forguson's contributions in the other three sections, and these accounts are generally placed so as to contrast with papers by philosophers who mis-interpret Austin's views.

Part I presents the orthodox account of what Austin did, and mostly did not, believe about the philosophical procedure of examining the minutiae of the everyday use of language, and these papers contrast with the contributions of Stuart Hampshire, who gives a not very radical characterisation of Austin's methodology. After a good deal of repetition—at least one contribution here is redundant—we have it firmly fixed in our minds that Austin's method consists of nothing more than (*a*) careful attention to ordinary use for the purpose of discovering important facts about the world, as reflected in linguistic distinctions, and (*b*) the examination of (some) traditional problem areas in philosophy in the light of these distinctions. But the Urmson-Warnock orthodoxy is not only tedious, it entirely fails to tell the reader what Austin's method really *was.* This point is made in the interesting contribution by Stanley Cavell who points out that it is ridiculous to rely on what Austin *says* about his method, since philosophers are notoriously bad at seeing just what they are doing, and Austin is no exception. The fact that Austin held no dogmatic views as to the correct methods of philosophy does not reduce the need for a clear account of the underlying assumptions which would explain the efficacy of his arguments. For, and this is the central point, Austinian arguments are very often highly effective in resolving *philosophical* problems, and yet it remains something of a mystery as to how the appeal to ordinary use can be *philosophically* significant. Cavell's complaint is not that the more or less orthodox discussion on Austin's method of procedure is far too restrictive and in need of constructive expansion, but that it actually obscures what Austin succeeded in doing. That Austin's arguments are indeed based on hidden and subtle principles of reasoning is apparent, especially in Part III which deals with *Sense and Sensibilia,* from the widespread failure to appreciate their force—a situation which is not always improved after a reinforced but orthodox statement of Austinian arguments which ignores his methodological presuppositions.

Part II starts with R. M. Chisholm's review of *Philosophical Papers* which is generally unsympathetic to Austin, by which I mean that he misconstrues many of Austin's views, as Irving Thalberg points out in his original contribution to this part. Chisholm believes that Austin does not appreciate the nature of

"Socratic" questions, which, for instance, need not be taken to entail scepticism. It would, however, be in the spirit of Austin to ask how Chisholm (and others like him) invests his questions with significance and, in particular, whether or not Chisholm's answers could have any intelligible application. Is not Chisholm's theory of evidence, like many such theories both in epistemology and the philosophy of science, open to the same criticisms as Ayer's sense-datum theory?

Also included are a useful (if orthodox) exposition by Forguson of Austin's philosophy of action (original as an English version), critical but constructive discussions by P. H. Nowell-Smith ('Ifs and Cans') and Irving Thalberg ('Austin on Abilities'), and what seems to me a very fair arbitration by Jon Wheatley (an original contribution) on the squabble over Truth. Part II also contains two gross mis-representations of Austin's method and views, the worst of which is roundly dealt with by Alan R. White ('Mentioning the Unmentionable').

While few would deny Austin's contributions to the philosophies of mind, action and moral discourse, his theory of speech acts is widely regarded as little more than the pointing out of a possible field of investigation. In Part IV Austin is several times attacked for no less than mis-using words in his *How to do things with Words*, notably by Walter Cerf who seems to regard the most valuable aspect of the book as its unconscious moving towards an existential phenomenology of language. For the rest, P. F. Strawson, Max Black, L. W. Forguson, L. Jonathan Cohen, and Mats Furberg argue over what can or cannot be saved intact from Austin's views, and conclude that there is relatively little.

Part III demonstrates best of all the failure of orthodox Austinian argument—Forguson's (original) rejection of Ayer's 'Has Austin Refuted Sense-Data?'—to establish the force of Austin's arguments. Although these are not, in *Sense and Sensibilia*, always fully cogent, as J. Bennett points out in 'Real', their general tendency is (correctly) seen to be devastating by Forguson, and yet Ayer does not, even in a rejoinder, see any such thing. Like R. J. Hirst (in his Critical Review) and Roderick Firth ('Austin's Argument from Illusion') he misses the point of most of Austin's arguments, and it is here, where Austin's method is most clearly *displayed*, that an explicit account is needed of why his appeal to ordinary use is effective. As one example of an Austinian methodological principle, I suggest the following: if the specification of an alleged problematic situation depends on the use of certain expressions 'E', and if, in *that* sort of situation, the use of 'E' would, judged by any ordinary standards of use, be entirely *inappropriate*, then the problem has not been shown to exist. This is, I take it, a reasonable principle, but, for the lack of seeing it, Ayer flouts it repeatedly.

All in all, this collection advances discussion of many topics which concerned Austin, but does not help us very much in understanding the philosophical essence of his method.

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JORDAN, Martin (1968) *New Shapes of Reality: Aspects of A. N. Whitehead's Philosophy*. London: Allen and Unwin. £1.75 (35*s*.) Pp. 184.

Most people's acquaintance with Whitehead's philosophy started with their reading of *Adventures of Ideas*; Mr Jordan's differs from theirs in that his didn't stop there. *Adventures of Ideas* is a great work of art, in which the European spirit was revealed by one of its maturest and most profound products. But I suspect that its wide acceptance as a sufficient indicator of Whitehead the philosopher may have given rise to the fashionable view that Whitehead "degenerated" from mathematician to "sage"; or even that he was a rather impressive but obscure cloud associated with the Edwardian sunset—in other words, the end, rather than the beginning, of an epoch. Even a hasty glance through the Preface of *Process and Reality* would dispel this complete misunderstanding of what Whitehead was about: Mr Jordan's writing does not suggest "enthusiasm" for extinct volcanoes.

Under the title *Interrelatedness* Jordan goes straight to the root of Whitehead's revolutionary approach, namely, his exposure of the *pseudo*-empiricism of the British "empiricists", culminating in Hume, derived from the fallacies of "simple location" and "misplaced concreteness", and the generation of the "problem of induction" (not so called by Jordan) by the common failure to recognize that "either there is something about the immediate occasion which affords knowledge of the past and the future or we are reduced to utter skepticism as to memory and 'induction' " (SMW, p. 62). This was hailed by Victor Lowe (*Understanding Whitehead*, p. 86, to which Jordan's book might provide a briefer and somewhat less technical introduction) as "one of the soundest remarks ever made on the problem of induction".

Basic to Whitehead's constructive thought is the doctrine of *feelings*: towards this dubious term Jordan rightly adopts a more tolerant attitude than is usual. If *every trace* of sentience is removed from 'feeling', the mental pole of every entity becomes as gratuitous as the physiologists' satisfaction with the "appearance" of consciousness at a certain level of complexity. In the next chapter Jordan stresses the complementary benefit of Whitehead's somewhat disturbing flexibility of terminology by urging that "from the 'playing down' of human consciousness the rest of the universe derives a majesty which, one feels, might not otherwise be its due" (p. 68). Better, he urges, to rename the Cartesian "substances" '*mechanism*' and '*life*'. "Thus freed from association with physics and psychology they appear more readily as the twin aspects of a single process" (p. 72).

The Spacetime Continuum covers both what is commonly connoted by that term and also Whitehead's cardinal distinction between 'causal efficacy' and 'presentational immediacy', with their relational concept 'symbolic reference'. Of the latter Jordan gives a brief but perceptive account; the brevity (except for the final section on 'extensive abstraction') introduces no distortion. The same can not be said about the former: there are many statements, that, if not positively wrong, are at least misleading unless qualified. None of them undermines the argument in this context; but are regrettable in a field where looseness of expression continues to generate many problems.

On the last of what I have called the 'reconstructive elements'—*God*—Jordan writes in a cautious—even apologetic—strain. A critical appreciation of his views both here and in the concluding pages of the book, would require a review to itself.

I have space left only for consideration of Jordan's emphasis (p. 141) on the "literary challenge of his philosophy . . . that there is perhaps a significance to be found in his later books considered solely as works of art". Forty years of "companionship" with these works has continuously strengthened the same conviction in myself, that, as Jordan notes (p. 153), "Whitehead loves phrases which seem at once obscure and darkly beautiful, but for him work as *terms of precision*" (italic mine). How hard it is to break through the traditional language barrier that Whitehead's *mathematical* understanding enabled him to do is shown by Jordan's occasional lapses into quite unWhiteheadian expressions such as, 'in time' (p. 30) and 'raw data' (p. 89); nor can he rid himself of that ghost of nineteenth century materialism still haunting the accounts delivered by scientists whose own work has destroyed its significance, the 'building *brick*'.

There are typographical slips on p. 29 and p. 148; 'Denis' should be 'Dennis' (p. 66), 'categorical' (p. 143) should be 'categoreal'. 'Substract', 'adaption', and 'ennunciate' are not to be found in my edition of the COD; but these, we are told, are times of rapid change. What of the signs of a "Whitehead revival" announced on the jacket? In a recent exceptionally long review in this journal, (**19** (1969) 359-71) of a symposium on *Time* the name of Whitehead, who made one of the most daring and fundamental approaches to the problem, does not appear. As Thomas Sprat might have observed, "*Revival* has not yet led to the *Extravagance* of *Enthusiasm*".

W. P. D. WIGHTMAN

YOLTON, John W. (1967) *Metaphysical Analysis*. London: George Allen & Unwin. £3.15 (63*s*.) Pp. xviii+216.

Professor Yolton believes that recent philosophers have made a number of false moves, which have resulted in their losing sight of "the proper way to analyse and understand philosophical problems". In this book he illustrates how such "metaphysical analysis" should properly be done, and criticises the "inadequate theory of meaning" whose acceptance underlies the failure of recent philosophers to do "metaphysical analysis" *comme il faut*.

The distinctive feature of "metaphysical analysis", which Yolton thinks recent philosophers have overlooked, is the requirement of both achieving an internal understanding of philosophical problems, while at the same time maintaining an external philosophical perspective. The metaphysical analyst must, in other words, seek to examine a philosophical question both internally, from the point of view of the conceptual system or philosophical language in which it arises, and externally, from the vantage point of his own conceptual system or philosophical language. Recent philosophers have ignored the requirement of internal understanding, seeking to explicate and criticise certain traditional theories entirely from an external point of view. In particular they have treated phenomenalism and sense-datum dualism in this way, committing what Yolton calls "the fallacy of translatability" by trying to formulate and criticise these theories in a philosophical language that embodies ontological assumptions which the phenomenalist and sense-datum dualist would find meaningless.

Phenomenalism, as expounded by Yolton, is a rather obscure doctrine. Viewed from the inside, it involves the claim that objects exist only in so far as they are objects of awareness, or objects for a self. To make sense of this dictum, however, and keep phenomenalism from falling into solipsism, we must view phenomenalism from an external, realist position. For an essential ingredient of phenomenalist ontology, the self, presupposes a subject-object distinction, and hence realism. One might be tempted to infer from this that phenomenalism *per se* is not just obscure, but incoherent, since the phenomenalist presupposes, for the very formulation of his theory, an assumption which he claims to be meaningless. For reasons I find hard to grasp, Yolton does not draw this conclusion; he concludes, rather, that "phenomenalism and realism as ontologies seem thus to be inextricably bound together, but only when we push phenomenalism to its fullest expression" (p. 30). Yolton criticises recent critics of phenomenalism—especially Warnock and Armstrong—for their refusal, or failure, to appreciate phenomenalism from the inside. Given their external, naïve realist point of view, they are precluded from providing a true account of phenomenalism, and therefore from directing significant objections against it. Mistakenly regarding phenomenalism as a linguistic or ontological alternative to, or variant upon, dualism or naïve realism, they condemn it on the irrelevant grounds of its failure to solve problems or make distinctions which, according to phenomenalism, cannot even be formulated. The force of this point is somewhat impaired by Yolton's admission that phenomenalism itself can be fully or consistently formulated only within a realist framework. It remains true, however, that many objections against phenomenalism—especially those based on considerations of "ordinary language"—show little more than that phenomenalists often use ordinary words in extraordinary ways. Like Yolton, I see nothing wrong with that; but I think that he under-values the importance of showing it.

In Part II Yolton considers sense-datum dualism. He begins by arguing that critics of the sense-datum theory, like critics of phenomenalism, have gone astray by giving a wholly external account of this theory. They have, in particular, neglected its basis in a Cartesian demand for certainty and a realist criterion of physicality. Yolton maintains that, given this demand and criterion, then "the sense-datum analysis is the best answer to the question of perception" (p. 56). But what is the question of perception? Is it not a question which the sense-datum theory is tailor-made to answer precisely because its very formulation presupposes the Cartesian methodology and dualist realism underlying this theory? Critics of the sense-datum theory have attacked that question; and I cannot see that Yolton even attempts to meet their attack. The rest of Part II contains three "metaphysical analyses" of the sense-datum theory: the first deals with the ontological status of Platonic sense-data, the second with Russell and Lewis on the transition from sense-experience to physical objects, and the third with Broad's theory of perception. Like other bits of the book—roughly half in all—these chapters are mostly reprinted from elsewhere.

Turning to Part III, where Yolton purports to exhibit the mistaken theory of meaning that underlies recent misinterpretations of phenomenalism and sense-datum dualism, we find a dismal farrago of arguments directed towards the conclusion that "reference is an integral part of meaning". He seems to think that proponents of the rules-of-use theory of meaning deny that explaining or grasping the meaning of an expression ever involves extra-linguistic reference because they take this to entail the false doctrine that the meaning of an expression is sometimes an extra-linguistic referent. One's confidence is shaken by his account of Russell on denoting (pp. 148–52), where he ignores the distinction between an expression's having meaning by itself, and its having meaning-in-context: contrary to Yolton, even after 1905 Russell seems to have identified meaning with referent. Confidence is not restored by his claim that modern logic has emended the traditional square of opposition "so that the logician makes no claims for existence" (p. 155). And confidence, not to mention comprehension, utterly collapses in the face of his argument that anyone who takes 'Odysseus was set ashore at Ithaca while sound asleep' to have meaning must take 'Odysseus' to have reference since if 'Odysseus' lacked reference then "the thought of the sentence would be incomplete" (pp. 159–60). After this misadventure in philosophical logic, Yolton goes on to expound a theory of objects, the point and content of which I could not understand; and to argue, no less opaquely, that a correspondence theory of truth applies to metaphysical statements.

For all its obscurity—Yolton's style is turgid and disjointed—this book contains a number of important points. In particular it is worth emphasising the need for an "internal understanding" of philosophical systems, and the danger of approaching such systems from an "external perspective". There are, however, dangers in adopting the "internal perspective". Of these, Yolton mentions only "loss of originality", by which I suppose he means failure, or inability, to break new philosophical ground. More serious, I should have thought, is the danger of losing one's critical acuity, or ability to appreciate the force of radical criticism. As mentioned above, I suspect that Yolton suffers from this in respect of recent criticisms of the sense-datum theory.

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RADNITSKY, Gerard (1968) *Contemporary Schools of Metascience*. Vol. 1: *Anglo-Saxon Schools of Metascience*. Vol. 2: *Continental Schools of Metascience*. Göteborg, Sweden: Akademiförlaget Göteborg. N.p. Pp. xxiv+200; xii+200.

These two volumes belong to a series, 'Studies in the Theory of Science', edited by Professor H. Törnebohm. Though they are designed to be read separately, if necessary, they in fact form a unity, since Dr Radnitsky's critique of logical empiricism, to which a major part of vol. 1 is devoted, is relevant to his preoccupations in vol. 2, where he is concerned mainly with the human sciences and the Continental philosophical tradition. In effect he seeks to mediate between logical empiricism, with its tendency to model sciences, even the human sciences, upon physics, and what he calls the "hermeneutic-dialectic" tradition of some Continental philosophers, who have started more from the end of philological, historical and literary research. Radnitsky is also much influenced by Popper, as we shall see. It should be noted that 'metascience' is used in this work to cover more than the philosophy of science: it includes also sociology and history of knowledge, study of research organisation and policy-making for research activities. Thus an important product of metascience is supposed to be advice and guidance as to research policies (hence some of the criticisms levelled at different accounts of science in these volumes relates to their alleged sterility from a heuristic point of view). It also needs stressing that Radnitsky stands in the European tradition in which many so-called "arts" enquiries (to use an English academic distinction) are treated as *already* being sciences—hence his disinclination to accept empiricist truncations of such enquiries. This is a major reason for his distaste for the ideal of unified science, since in the English-speaking *milieu* it is liable to lead to "pure science" imperialism.

Briefly, Radnitsky's thesis is as follows. Logical empiricism provides an ideal of science which is naturalistic and devoted to regularity-explanations. On the other hand, the "hermeneutic-dialectic" tradition (Radnitsky draws heavily here on the work of Gadamer, Apel and Habermas) has been more concerned with *Verstehen* than with explanations, and with problems of the degree to which criticism has to enter into hermeneutics—an issue arising from scepticism about the possibility of the interpreter himself adopting an ahistorical stance in relation to the tradition, historical events, etc., which he is interpreting. Crudely, causal explanations become relevant in historical enquiry when "good-reason-explanations" or "rational explanations" (in William Dray's terminology) do not work. But here the concept of rationality needs, for obvious reasons, to be seen in the context of hermeneutic enquiry (otherwise we merely project our own norms on to other groups, etc.). Thus the human sciences require a complementary approach, from the side of hermeneutical understanding and from the side of naturalistic explanation. Or quasi-naturalistic explanation, as Radnitsky prefers to call it, since naturalism is associated with a totalistic empiricism. (I here reproduce some of his jargon, which is formidable and somewhat barbaric, though it should not blind us to what he is wanting to say.) But since also the human sciences are in a sense part of the processes which they seek to understand, they cannot in a facile manner be value-neutral. Indeed the reduction of sociology to a technical, empirical branch of study itself makes it subservient to political and social ends entrenched already in society. This is a result of mistaking the real lesson of empiricism, viz. that claims need to be checked, and artificially constricts the scope of enquiry. Radnitsky finds in Popper a means of allowing criticism to enter the human sciences, in conjunction with hermeneutics, without selling the pass to ideological subjectivism. For Popperian criticism is guided by the ideal of the open society, in which dialogue takes place; dogmatism rules out dialogue and thus the possibility of our treating the dogmatists by the method of *Verstehen*: they become quasi-natural objects, and so lose a very intimate stake in the human sciences. In the exposition of his general theory, Radnitsky makes use of the example of psychoanalysis, in which he stresses the role of hermeneutics and the bringing about of "self-understanding" (he criticises some critics of psychoanalysis for construing it primarily as a system of *explanations*).

Radnitsky's intentions seem to me to be good: the question is how successfully they are realised—the intentions, namely, of relating "arts" methodology to that of science (in the Anglo-Saxon sense), and of relating scientific method to the communities and organisations in which it applied. On the first score, it seems as if Radnitsky overstresses the centrality of hermeneutics as conceived in the German philological tradition, which tends to neglect the analogous questions of interpretation thrown up by social anthropology. Just because much of the philological tradition is classical in orientation it turns hermeneutics into something more than a method of uncovering the meaning of documents, etc., and gets concerned with a more ambiguous operation, viz. presenting the "meaning" of the tradition for us today. It is in this context that Radnitsky wants to integrate a critical approach with pure hermeneutics. It is also in this context that he draws on the psychoanalytic model of the human sciences, for they aim at "self-understanding" (e.g. reflection on the tradition gives us an understanding of our condition, etc.). It seems to me that there are grave ambiguities in all this. The anthropologist working among the Coorgs is primarily concerned with understanding the Coorgs: his results may be relevant to anthropological theory, e.g. functionalism, which can in principle throw light on the structures of the investigator's society, but this is not aptly described as unravelling the meaning of the Coorgs for us. In brief there is a lot of obscurity about who "we" are supposed to be. Unless this is specified, there is doubt as to the role of criticism and self-understanding in Radnitsky's scheme.

This problem leaks over into the question of the actual social role of research and understanding—a question which Radnitsky is committed to exploring in view of the wide scope of metascience (compared with philosophy of science). To give a case where more analysis is needed: it does not seem to me to be the major criticism of value-free empirical sociology that it is forced to adopt a technical role in society. The major criticism of many empirical studies is that they are aimless, because not geared to a theoretical framework. Unfortunately much which passes for sociological theory is insufficiently free from ideological assumptions. Admittedly Radnitsky stresses the role of Popperian criticism in uncovering ideologies; but this may mean that there is a certain inconsistency in his approach to the social sciences.

The book abounds in misprints, is full of difficult jargon and is thereby not strong on clear exposition. But it poses some important questions and contains many interesting leads worth following up.

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O'Brien, D. (1969) *Empedocles' Cosmic Cycle: A Reconstruction from the Fragments and Secondary Sources.* Cambridge: Cambridge University Press. £5.00 (100*s*.). Pp. x+459.

This book is a specialised treatment of a specialised theme. Out of about 150 extant fragments of Empedocles' actual poems, perhaps a dozen refer directly to that cosmic cycle on which modern scholarship has concentrated so much attention. In his introduction to the cosmological fragments Burnet pointed out that the whole of our material constitutes perhaps one fifth of what Empedocles had to say: in such a case does the plain man, following Millerd, regard imagination in Empedocles as a more powerful force than logic, or does he—in view of constant expert disagreement on points of detail—regard any reconstruction of the system as potentially a house of cards? That Empedocles described a cycle of some kind—and it is in Raven's words "the most peculiar feature in his whole system"—is not in doubt: what has been disputed, in ancient as in modern times, is the detail of the cyclic system.

Dr. O'Brien's new book is a full-scale vindication of the majority view associated with Zeller and Burnet and outlined with admirable clarity in his first chapter. The two cosmic forces of Love and Strife control, each in turn and in endlessly alternating cycles, the elements of earth, air, fire and water which make up the whole of reality. Love unifies things, Strife separates them. A world like ours arises at two periods in the cycle, one under increasing Strife and the other under increasing Love. In his last chapter O'Brien allows himself a relaxed and interesting treatment of Empedocles' place in the development of fifth century thought, relating him particularly to Parmenides and Plato.

In the body of the book O'Brien comes to grips with his main problems: his treatment here is far from relaxed, constricted in style and method, and uncompromisingly technical. He himself describes the evidence as "fragmented and intertwined" (p. 58), but there are passages where the reader may feel that the order of procedure implied in the book's subtitle is not fulfilled, and that the author has not achieved the clarity and simplicity at which he so rightly aims. This feeling of the trees rather than the wood may be a by-product of the author's obvious learning. The Russian entries in the bibliography are translated for the grateful reader, but otherwise he is assumed to cope equally well with Greek, Latin, Italian, German and French. The fact that crucial passages are not translated probably seems all the odder because the Cambridge imprint has recently done so much to make Greek thought accessible to the interested but Greekless reader.

In his detailed analysis of the main problems the author brings to bear an impressive breadth of reading and a capacity for intricate argument, together with many original ideas. An introductory chapter on rest and movement makes it clear that "the association of plurality and movement, of unity and rest, is a common assumption of the fifth century", and fundamental to an understanding of Empedocles. This leads to a short treatment of speed, associated with Strife, and a full treatment of time, where it is argued that the time of total Love balances the time of plurality and movement, and in particular that the time of total Strife is not a period but a transitional moment. His discussion of the position and shape of Love and Strife during the phases of the cycle concludes convincingly, and against the usual view, that Love is always central to the world and always surrounded by Strife. A short chapter then argues that the separated elements under total Strife must be arranged in concentric spheres. These chapters (2–7) and chapter 9 on the relation of zoogonical stages to the cosmic cycle contain a great deal of radical argument (much of it polemical) and show a magisterial acquaintance with the vagaries of scholarly opinion.

But it is only in chapter 8, on the cyclic system, that O'Brien raises the question of whether there were in fact (or rather in Empedocles' cosmology) two alternating worlds of "increasing Love" and "increasing Strife"—the basic assumption of the whole book. Simplicius had treated Love and Strife as permanently at war in one world: Karsten (1838) argued for a permanent world order, as did Tannery (1887), von Arnim (1902), Kranz (1912) and Freeman (1946). And very recently, after O'Brien had completed this book, Bollack, Hoelscher and Solmsen have argued strongly against the alternate worlds. So the basic question has been re-opened, and the reader may doubt that the rewritten chapter 8 is enough to lay the dust.

Nearly half of the book is made up of appendices, two very full indices, and a critical bibliography which aims to include (and almost certainly does) all books and articles devoted exclusively to Empedocles from Sturz's edition of 1805 to 1965. This is a very valuable feature of the book and an achievement in itself. Physically, the book has the excellent typography and binding that one expects of the Cambridge Classical Studies: it is sad that it is so expensive, because it is a book that every serious student of Empedocles should be able to consult.

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STRAUS, E. W., NATANSON, M. & EY, H. (Ed. Natanson). (1969) *Psychiatry and Philosophy*. Berlin: Springer Verlag. DM. 32. Pp. xii+161.

This book consists of three parts, each of which was originally a chapter from a much larger multi-volume psychiatric text *Psychiatrie der Gegenwart* (Present-day Psychiatry) published in 1963 and edited by W. H. Gruhle, R. Jung, W. Mayer-Gross and M. Müller. This new selection is somewhat a bibliographer's problem in that although Natanson is described as the book's editor on the title page, the preface is signed by both Straus and Natanson and to add to the confusion the general acknowledgements are by Straus.

The first part, entitled *Psychiatry and Philosophy* is by Straus. To those acquainted with the field of phenomenological and existential psychology the name and reputation of Straus will be very familiar. Straus left Germany in 1938 for the United States and during his long life developed an authority and regard that has made him a leader in his field. During the last ten years, translations of his books and articles have become increasingly available to interested readers. In consequence, we can expect and we do obtain an authoritative presentation of the phenomenological approach to problems relating to the borderlands of Psychiatry and Philosophy. Unfortunately it is not something that can be recommended for anyone wishing to learn about what has been called the "third force in Psychology". Written by a specialist in a language and style requiring substantial previous reading in phenomenological psychology, it is replete with terms and modes of expression which would be extremely difficult for the uninitiated. The occasional attempt to explain a term is only half-hearted and parts of this section become almost like a philosophical speaking-with-tongues. For example, "Sensory experience is embedded within this relation to the Allon. The Allon is present to me as body-subject; within my active resistance to gravity the Allon is experienced in its own 'heft' and substantiality as resistant, even while in contraposition it is grasped in its suchness as object" (p. 76). In relation to this example, an attempt has been made (on p. 25) to give some meaning to the term 'Allon' but it is of little help to the inexperienced.

Natanson's section is concerned with *Philosophy and Psychiatry*, and in a relatively short space (pp. 85–110) presents a phenomenological examination of philosophical questions directed to psychiatry. It deals with the "World of every-day life", "Normalcy", "The Morbid", "Communication" and "Etiology and Therapy". In contrast with Straus's section it is much more understandable but at times the author cannot resist the special terminology and abstract presentation so characteristic of such work: "If the capacity to typify is the epistemological *a priori* of our common world of mundane existence, the implicit sociality of all human action may be said to be the ontological *a priori* of the *Lebenswelt*" (p. 100). No one questions the need for specialist languages and terminologies. The description of subjective states and modes of being must necessarily be an unsatisfactory translation from experience into verbal communication to others but the reviewer questions the form in which it is done in this type of writing. One cannot but think that there is a predilection for abstract terminology for its own sake. If the reviewer is wrong in this view, it must be explained to him in a way that really removes his suspicions.

The final section by Ey is entitled *Outline of an Organo-dynamic Conception of the Structure, Nosography and Pathogenesis of Mental Diseases*. Written in the great French tradition of Psychiatric literature, it is a presentation of the application of neurological models of the evolution and dissolution of the nervous system to mental illness. Explicitly influenced by Hughlings Jackson and Janet, Ey presents a case for the structural evolution of mental life and its hierarchical organization in a form that will be probably more congenial to his new audience than to his original one. Ey feels that such a model sidesteps the inherent difficulties of dualism and monism. As a practical alternative this seems to be the case but his arguments on a philosophical basis are unfortunately too brief. The scholarship and breadth of reading shown by Ey is very impressive and serves as a welcome reminder of the enormous contribution made by European writers to the growth of psychiatry.

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SCHMIDT, Siegfried J. (1969) *Bedeutung und Begriff: Zur Fundierung einer sprachphilosophischen Semantik* (*Wissenschaftstheorie, Wissenschaft und Philosophie*, Band III; herausgegeben von Simon Moser und Siegfried J. Schmidt). Braunschweig, Germany: Vieweg. DM 22.80, Paperback DM 16.80. Pp. xiii+176.

This is a heavy book. My review copy tipped the scales at 11½ ounces, which is a lot for a paperback of less than 200 pages. Not that it was very solidly built—it fell to pieces during the first reading. Perhaps, however, I should give the binders the benefit of the doubt and admit that the process would be better described as "the first ploughing-through", for much of the text is heavy as well, which is not to say that any of it is weighty.

It is the hope of the author, and of Peter Hartmann, who writes a short introduction to the book, that it will show the way to closer co-operation between philosophy and linguistics, but in this hope they are surely due to be disappointed, and for a very general and fundamental reason. The philosophical analysis of language and meaning, like most thoroughly controversial areas, has not yet developed any terminology which is both precise and generally accepted. Consequently, an author who wishes to advance the state of knowledge rather than just swell the bibliography must take extreme care to make his meaning clear, since he is otherwise unlikely to convey much that is sharp enough to have a definite truth-value. This Schmidt appears not to realise; or if he does, he has mis-estimated the quality of his precautions against it.

Broadly speaking, there are two sorts of writing involved. One is reporting of the views of other thinkers, and this, though extensive, is often superficial and leaves one feeling no better off for having heard it all, if not positively puzzled. The other consists of dizzily abstract theorising in the kind of German which has generated the myth that the language is somehow inherently and inescapably ponderous. One cries out for a few concrete applications of the terminology to tighten one's grasp of its meaning. I instance a short passage (p. 6) which illustrates to some degree both of these features. Schmidt quotes a sentence of H. Delius, which I translate (without much regard for style) as follows:

> A question in the theory of meaning is a question about something which does not appear as a datum during the unreflective performance of speaking until the question about it is asked . . . the theoretical question about what we mean when we speak makes our speech the sort of thing of which it makes sense to ask that question.

This, Schmidt tells us, explains exactly why he calls questions in the theory of meaning 'meta-questions'. Now what Delius may have said in explanation I do not know, but just on its own, as Schmidt presents it, it is extremely obscure. It seems that if a form of behaviour is meaningful, in the semantic sense, it must make sense to ask about its meaningfulness; and surely we are not supposed to think that language was not meaningful before it was the object of theoretical investigation? Or does the fact that in the everyday pushing-around of physical objects no questions about their internal construction force themselves on the uninquiring consciousness make the questions that physicists ask "meta-questions" in Schmidt's sense?

One more example (p. 89): with the remark that his terminology largely follows that of Martinet, the author gives the definitions:

'Morphem': jedes grammatikalische bedeutungsfähige Monem (etwa -e in hab-e).

'Monem': jedes lexikalische bedeutungshaltige Morphem (z.B. hab-).

The logic of this is very puzzling. The 'e' of 'habe' is said to be a morpheme. To be a morpheme it must be a certain sort of moneme, and to be a moneme it must be "lexikalisch bedeutungshaltig". Is the 'e' "lexikalisch bedeutungshaltig"? Intuitively one would say not. Perhaps it is, but in that case just what is 'lexikalisch bedeutungshaltig' being used to mean here? Further, does it not follow from the definitions that any morpheme (or moneme) which is both "grammatikalisch bedeutungsfähig" and "lexikalisch bedeutungshaltig" is also a moneme (or a morpheme), and that anything which is not both "grammatikalisch bedeutungsfähig" and "lexikalisch bedeutungshaltig" is neither a morpheme nor a moneme? Why then two terms, and what is gained over simply telling the reader to use his intuitions about how words divide up into significant bits, since he is going to have to do that anyway if he is ever to decide, on this definition, whether something is (*a*) both a moneme and a morpheme, or (*b*) neither. Here, and elsewhere, one is left wondering.

Bedeutung und Begriff bears witness to considerable scholarship and breadth of reading on the part of its author. But a book can of course display these qualities without making any genuine contribution to knowledge, and unless my impressions have deceived me badly this is precisely such a book.

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MANNERS, Robert A. & KAPLAN, David, Eds. (1969) *Theory in Anthropology: A Source Book.* London: Routledge. £6.30 (126*s*.). Pp. xii+578.

Of the making of readers there is no end. The market appears to be flooded and something more than just the inclusion of good material is needed before an anthology of previously published articles can be recommended. The editors of this volume seem to agree, for they say "that it is now very difficult to assemble a collection which does not come out looking like a reader of readers . . . any new collection of old writings must justify itself". The justification they provide is, alas, hardly adequate. They argue that many pieces of writing relevant to courses in anthropological theory are scattered in numerous publications, some of which are difficult for the student to obtain. Agreed; but at six guineas a copy, this anthology does little to make them accessible to students. Despite the price, the standard of proof reading is poor. The size of the book, and presumably its price, is inflated by the inclusion of many items that are easy to find. Of the 52 items in the collection, 22 come from anthropological and sociological journals, and of these only two (*América Indígena* and the *Rhodes-Livingstone Institute Journal*) are likely to be unobtainable in an average department of social anthropology. Seven items come from philosophical journals that should be available elsewhere in a university. Five are taken from books, all of them standard works. Only the fourteen items taken from symposia and conference proceedings are comparatively scarce. If these alone had been made into a volume, then maybe the student with limited library facilities would have been helped. The inclusion of articles from journals like the *American Anthropologist* and the *American Journal of Sociology* can help those students who are so poorly provided for that they do not have access to these basic journals; but these are the students whose departmental libraries will probably be unable to afford the anthology.

The second and major argument advanced by the editors to justify their anthology is that it offered an opportunity to examine and evaluate how anthropological theory "might be defined, how it developed, how it was applied, and how one went about confirming or disconfirming it". I presume there would be a case for reproducing a previously published article, even though it might be readily accessible, if it formed an essential step in the development of some argument or exposition in which the editors were interested. Unfortunately, these arguments or expositions, if there are any, never emerge. There are no editorial introductions to the items or even to sections of the anthology. The editors say these were omitted because they believe that "the selections speak for themselves and do not require any lengthy exegeses". If they speak at all, it must be very softly. The last three items, an early paper of Lévi-Strauss and two commentaries on his work, hang together, but otherwise there is no obvious connection between the five or so articles which make up the various sections, and the reader has to attempt his own reconciliation of the conflicting explanations and emphases he encounters. This may be good for him; but without some comment from the editors about the reasons governing their choice of articles, the reader cannot easily see how the articles before him relate to the wider corpus of writing that makes up theory in anthropology. Are the selected articles a random sample or has the selection been purposive? If, as we hope, it has been purposive, what was the purpose? The editors have suppressed all bibliographic references in almost all the articles, making it even harder for the reader to see how they relate to other works.

The only evidence for the rationale of the selection is to be found in an eleven-page editorial introduction, entitled 'Notes on theory and non-theory in anthropology'. This contains a balanced discussion of the place of theories and generalizations in anthropology, and of the special difficulties that hinder the development of mature theory in social science. This is a good sermon, but it fails to provide any implicit criteria for selecting items for inclusion in the anthology or any rules for interpreting the items that are included; it fails to grind any axes.

Not surprisingly, the resultant anthology is rather dull. Substantive, potentially testable, propositions begin to emerge about halfway through with a group of four articles on social evolution, followed by a variety of interesting but uncoordinated propositions on the interconnections of social structure, personality, ecology and language. The last section contains a few articles on what the editors call "the new ethnography", i.e. componential analysis and ethnoscience. The first half of the collection is mainly devoted to meta-theoretical questions, such as the meaning of 'explanation' in social science, and the use of dynamic (i.e. diachronic) models, together with a mixed bag of articles on "Methodology" in which I could discern no common ground at all.

The most surprising feature of the anthology is its treatment of sociology. Despite its title, the anthology ignores physical anthropology and prehistoric archaeology and confines itself to cultural and/or social anthropology. There is a widely-held view that although social anthropology and sociology may differ in practice, the former concentrating on the study of tribal and peasant societies and the latter on industrial societies, there is no difference between the two disciplines at the level of theory; Marx, Weber and Durkheim form an orthodox trinity for both sets of believers. Hence in an anthology devoted to theory rather than practice in anthropology, we might expect to find the distinction between anthropology and sociology hard to draw. Instead, we find sociology and sociological theory essentially ignored. Only five of the items are contributed by sociologists, and of these two are about the work of anthropologists. In the same parochial outlook, we find, with the solitary exception of Lévi-Strauss, that all the authors were trained in, or work in, the United States or Britain. Thus blinkered, the anthology inevitably gives the impression that theory in anthropology is still at a neolithic stage, reminiscent of slash-and-burn systems of agriculture, as noted by the editors, following Anthony Wallace: "After cultivating a (theoretical) field for a while, the natives move on to a new one and let the bush take over; then they return. . . ." Perhaps sociology could lend them a few bull-dozers and bags of super-phosphate, to make cumulative cultivation possible.

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HARVEY, David (1969) *Explanation in Geography*. London: Edward Arnold. £3.25 (65*s*.) Pp. xx+521.

Geography, very broadly speaking, sets out to provide an explanation for the distribution and changing nature of the phenomena at the earth's surface. The critical difficulty in twentieth-century geography has been to decide what shall constitute—in Nagel's words—"a systematic and responsibly-supported explanation" in any one section of the subject and, more important, whether the same criterion must apply throughout the discipline. This dilemma has led to a prolonged discussion of the value and validity of different approaches and, in particular, of the relation between quantification and the "scientific respectability" of explanations. Such a debate has been going on, of course, in allied fields—see for example, many of the essays in *The Fabric of Geology* (edited by C. Albritton, 1963)—but the diverse subject matter of geography and the apparent differences in the problems facing the physical and human sides of the discipline would seem to have increased the difficulties of the geographers' discussion.

At this point it must be admitted that too much of this debate in recent years has been carried on in surprising ignorance of views on the nature of scientific explanation such as those of Nagel, Braithwaite and others. One of Harvey's achievements, from the practical point of view, is to provide an admirable survey of the major ideas of modern philosophers of science which should prove accessible to the student and teacher of geography alike. Although it is possible to criticise this section of the book either on the grounds that, for many geographers, the philosophical argument may prove at times to have been pursued too far for comprehension or, I suspect, that for the philosopher of science the summary is over-simple, yet it is overall of great value and the elegant extension of Toulmin's analogy between theory and map given in Chapter 12 would seem of interest to both groups of readers.

Explanation in Geography is not, however, a mere elementary text in the philosophy of science, for geographers. Harvey appears to have produced the most authoritative and helpful statement of "The Nature of Geography" since the publication of Richard Hartshorne's classic of that title in 1939. A measure of the changes within the discipline in that thirty-year period is the far greater emphasis which Harvey places on the means whereby geographers may arrive at explanations: the majority of the book consists of an examination of the languages available for the expression of geographic problems and the relations between the choice of language and the processes of model-building, law-making and theorising. It is regrettably true at present that, as Harvey says "most of our theorising is inexplicit and fuzzy": if nothing else, his work should force the geographical community to a more rigorous examination of the theoretical content of many of its most cherished models. Such an exercise may prove arid, but one would hope that it will rather give rise to some of those intuitive, imaginative leaps without which no branch of knowledge can really progress.

In brief, this is a fascinating, taxing and extremely important work. It is also a personal statement and therefore an invitation to discussion: it is to be hoped that, unlike much geographic deliberation, the discussion will prove fruitful in "real world" terms.

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MEDAWAR, P. B. (1969) *Induction and Intuition in Scientific Thought.* London: Methuen. 80p. (16*s.*) hardback, 35p. (7*s.*) paper. Pp. ix—62.

Sir Peter Medawar has written a very nice little book. It is based upon his three Jayne Lectures delivered to the American Philosophical Society in 1968, and covers exactly what its title says: the relative importance of induction and intuition in science. The title, however, is misleading in that it suggests a heavyweight work on scientific methodology; whereas Medawar's book has more the nature of a *jeu d'esprit.* It might be seen as a postscript to his earlier collection of essays, *The Art of the Soluble.* It has very little originality (in fact, it doesn't profess to have any)—perhaps Medawar's best claim to originality is in championing the claims of Claude Bernard as a major philosopher of science.

Medawar's thesis is that inductivism is inadequate as an account of what scientists do. It is inadequate, he urges, on seven grounds: (1) The inductivist believes that the actual work of the scientist in creating a new theory can be logically reconstructed. Hence the inductivist is hopeful that scientific theories might one day be created by the mechanical application of rules (if only we knew the right rules). Although I largely agree with what Medawar says, he sometimes seems to be too zealous in defending the mystery of scientific creation, rather as if he regarded science as in some way degraded if it were fully explicable. (2) The inductivist believes in the possibility and indeed in the primacy of uninterpreted data. Medawar believes that "innocent, unbiased observation is a myth" (p. 28) (he even quotes Nietzsche in his support) but does not say how far he wants to go in this direction: does he believe, like Hanson, that every perception is theory loaded? (3) Although inductivism speaks of "relevant information" it has no criteria of what constitutes relevant information without presupposing some theory or other. (4) If inductivism is right theories must be accepted outright or rejected outright; it leaves no room for the modification of theories. I'm far from certain that this is so. The inductivist is just as well equipped to modify his theories to account for awkward facts as the hypothetico-deductivist. The only case in which this isn't so is when his theory consists entirely of universal generalisations. (5) Inductivism fails to explain the prevalence of error in science. (6) It leaves no room for luck in scientific discovery.

(7) It regards experiments merely as means of generating facts and ignores their critical function. These are Medawar's reasons for rejecting inductivism; they are not all cogent objections, nor, I think, are they the best objections to be made.

The way Medawar overcomes them is by assuming that intuition plays a far greater role in scientific discovery than the inductivist allows. With this I am in complete agreement. Science proceeds by making intelligent guesses and then testing them. The two processes, of guessing and testing, are completely distinct. Scientific progress, Medawar says, can be seen as a dialogue between the imagination and the critical faculties. This is rather similar to a point made by Ernest Jones in his biography of Freud about the psychology of scientific genius. Great scientists are often imaginative to the point of credulity about some things and rigorously sceptical about others. One thinks of Kepler's belief in astrology; Newton's biblical studies; Einstein's cosmic repulsion (and, some would say, his unified field theory) and Freud's own number mysticism. In this way the psychology and methodology of science go hand-in-hand.

I'm not sure that Medawar's doctrine can give satisfactory answers to the points he made against inductivism. For instance, he still has no criteria for "relevant information", except that the scientist intuits what is relevant, which seems rather lame. Nonetheless I'm sure that his account is more satisfactory than the one it supersedes. His book is not thoroughly argued and will convince no-one who wasn't convinced by Popper's *The Logic of Scientific Discovery.* Nonetheless it is agreeable and very pleasantly written.

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HOLYOKE, T. & HAHN, S. W. Eds. (1968) *Foundations of Mathematics: Symposium Papers Commemorating the Sixtieth Birthday of Kurt Gödel.* Berlin: Springer Verlag. DM. 39. Pp. 228.

This volume contains the papers contributed at a Festschrift Symposium held during the 75th anniversary meeting of the Ohio Academy of Sciences, 1966, to commemorate the 60th birthday of Dr Kurt Gödel and the 35th anniversary of the publication of his celebrated theorems on undecidability.

The first paper—'Realism as a philosophy of mathematics' by S. F. Barker—is the only non-mathematical one in the book. Barker examines several objections to the realist philosophy of mathematics espoused by Gödel.

More than half of the book—four papers by A. Lévy, G. Sacks, R. Solovay, G. Takeuti—is devoted to technicalities of axiomatic set theory. Amongst the five remaining papers is the gem of this collection—a short, sweet paper by H. B. Curry presenting a result about combinatory logic. A survey article by B. Meltzer on theorem proving by computer reveals the paucity of ideas in this area. H. Rubin presents a new axiomatisation of probability theory within the framework of NBG set theory which, it is claimed, overcomes certain alleged unsatisfactory features of the standard Kolmogorov formulation. Finally there is a 66-page paper by E. Wette written in that style of German where the nesting of dependent clauses does not always satisfy the descending chain condition. The contents of this paper will remain a mystery to anyone whose native tongue is not German.

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SENGUPTA, Pradip Kumar (1968) *Demonstration and Logical Truth.* Calcutta: Academic Publishers. £1.50 (30*s*.). Pp. x+276.

Sengupta's concern is with the concept of 'proof' and the need for a reasonable interpretation of 'formal validity' of inference, and sums up his book as "from Aristotle to Carnap through Bradley". Inference is initially defined as an intellectual operation (recognised as such) which involves an element of belief (to distinguish it from imagination) and an explicit sense of 'therefore' (following Johnson and to distinguish it from association) taking us "necessarily" from the ground to the result (to distinguish it from perception) furnishing some new truths not contained within the very ground itself (to distinguish it from repetition). As an attempted solution to the so-called necessity-novelty paradox, three forms of novelty are considered; Keynes' verbal novelty, subjective novelty and Mill's objective novelty. But Sengupta's main concern is with the ambiguity of 'contain' and 'follow from', where the premisses may be said to contain the conclusion, or the conclusion to follow from the premisses; i.e. with entailment. Inferences on the basis of entailment are free of the paradox. To Aristotle's equating inference with demonstration with syllogism, Sengupta lists (i) Cook Wilson's criticisms, (ii) Bradley's view that every demonstration is not an inference and (iii) Mill's view that inference is not merely demonstration, it is proof. Considering inference as proof, three preconditions of proof are enumerated: self-evident truths, intellectual passage from these to new truths and new truths which become self-evident on the basis of the old truths but not alone. Sengupta emphasises that the new truth must follow "necessarily" from the old truth, and quotes Johnson's "epistemic" and "constitutive" conditions for the validity of an inference, before discussing Mill and Bradley. The evaluation of Mill's objections to the inferential value of syllogism and Bradley's objections to syllogism as such follow Keynes, Johnson and Cohen and Nagel. Mill's theory of inference is then discussed at length. Hobhouse's criticisms of Bradley's objections are largely dismissed before a consideration of the fundamentals and experimental methods (Bacon) of Mill's theory of induction. Sengupta's critical examination of Mill follows Bradley's objections to induction as such and to the inductive methods of Mill in particular. Carnap and Popper are mentioned briefly.

Chapters 6 and 7 deal with Bradleyan formalism. For Bradley an inference is always an ideal construction resulting in the perception of a new connection which necessarily follows from the construction. And the construction which gives us the premisses is to be made always on the basis of some common link between the elements and this common link is the point of identity. Bradley's two conditions of inference are that it is impossible to reason except upon the basis of identity and that one of the premisses at least must be universal. On the principle of the identity of indiscernibles, Sengupta cites Leibniz and Russell.

For Bradley, (i) inference is an activity; (ii) not any and every activity can be called inference; (iii) inference is necessary and universal and (iv) inference gives us (on the basis of some truth) some new truth which is not apprehended when we are confined within the limits of our premisses. Sengupta sums up the main points of Bradleyan formalism as (i) Logic is not an art of reasoning. It is an abstract science which endeavours only to construct a general outline of the structure of inference; (ii) There is no limit as to the number of premisses in inference. There is also no fixed order of the premisses; (iii) The principle of identity which works in the background of inference is a purely formal one and has been sharply distinguished by Bradley from the metaphysical use of the same; (iv) The needed universal character of the premisses is to be understood not in terms of the denotative and existential abstract nature of the premisses concerned. Every universal judgment is hypothetical. Different meanings of identity are then discussed and identity distinguished from similarity.

Using a definition of 'logical truth' due to Carnap, Sengupta attempts to explain 'proof' in terms of necessity, and enumerates Bradley's arguments to show that necessity is hypothetical. Chapter 8, section 3, elaborates the formal language, propositional variables, connectives, wellformedness, two-valued truth tables, tautologies etc: considers Strawson, Faris and Russell and ends briefly with entailment-implication distinctions. Sengupta elaborates Johnson's view that the paradoxes of material implication are harmless and proposes an analysis in terms of entailment. Sengupta considers that by entailment we mean demonstrability and that entailment should be defined in terms not of material implication but of logical implication. The section dealing with logical implication is confusing, allowing for example L-implication between 'red' and 'coloured', as well as between propositions. The paradoxes of modal logic are unfortunately not considered, nor are the more well-known paradox-free formal entailment systems.

It is indeed unfortunate that this book contains so many printing errors, too numerous to list here in detail.

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CHER, Nicholas (1969) *Many-valued Logic.* New York: McGraw-Hill.
$8.95. Pp. xv+359.

eat of Professor Rescher's book is a 200 page survey of many-valued logic,
led by a short but interesting historical conspectus and rounded off by 100
of bibliography (there are in fact three bibliographies, the same entries
listed first by date, then by author, then by topic). As Rescher says, any-
dertaking such a survey is likely to have to concentrate either on the more
mathematical aspects of the subject, or on its applications, or on the
tical considerations involved in interpreting many-valued logics. His
eference is for the last of these, and his semantical considerations are of
ds. Firstly he believes that if a system is to be taken seriously it must
n in certain minimal respects to our pre-systematic understanding of
gic is all about. A great deal of his discussion is therefore concerned with

the minimum requirements for a connective to express a "reasonable" implication or a "well-behaved" negation, etc., and he likewise prefers to frame his generalisations about systems in terms of "viable" or "plausible" systems. His second thesis is that the values of a many-valued logic ought to be explicable in terms of the basic truth-values, truth and falsity. For example, the values of an 8-valued logic might correspond to the eight triple combinations of truth or falsity of a proposition in Euclidean/Riemannian/Lobatchevskian geometry; and Rescher's survey is largely concerned with constructions of a comparable kind.

As the author says, at present there exists no well-rounded introduction to many-valued logic, and someone who has not tried to fill the gap will hesitate before criticising the choice of material made by someone who has. There are however two omissions which are surprising in terms of Professor Rescher's own programme. In his philosophical chapter on relativism in logic he concludes that although there is room for choice between different logical systems, it is a choice constrained by pragmatic "instrumentalistic" considerations about the purposes the systems are intended to serve. This instrumentalist approach would suggest an emphasis on logical deducibility rather than logical theoremhood in isolation, but Rescher does not consider deducibility apart from the derivation of logical theorems, not even to require that rules of inference should be truth-preserving (p. 71). Given the stress on pragmatic considerations my other regret is that Professor Rescher does not say more about the applications that have actually been proposed for many-valued logics. Of the applications most likely to interest readers of this *Journal*, for example, Reichenbach's logic for quantum theory is given one page, while Körner's logic of inexact concepts is not mentioned at all. The suggestion of a certain writer that with a 3-valued logic one could simultaneously adhere to contradictory physical theories is described as "not worked out in detail sufficient to underwrite a favorable evaluation"—an astonishingly tolerant verdict coming from someone who rightly believes that logical systems should have at least some relation to our pre-systematic understanding of "what logic is all about".

Addenda and corrigenda. (1) It is not true that Kleene's is the strongest normal
regular 3-valued logic (p. 58), for the singulary connective whose table is
column of T's is not present in it. (2) $\alpha \,\&\, \beta \supset \alpha$ is not a theorem of the system
pages 142–3. Since on the contrary $\alpha \,\&\, {\sim}\alpha$ is a theorem, one must doubt the cl
that '&' here can be squared with the orthodox conception of conjunction. (3
F-split system on page 341 needs amending if it is to satisfy its description
axioms: the tables for implication and equivalence should have T ever
except in the first row and column. (4) The bibliography unfortunately om
mathematical works of E. V. Huntington, which Rescher would have fou
ticularly relevant to his semantical concerns. The omissions on the philos
side are more systematic—the entire literature on truth-value gaps and the
of descriptions, for example. (5) "The crucial distinction between meani
use" on page 18 should be the crucial distinction between mention and us

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KAHANE, Howard (1969) *Logic and Philosophy: A Modern Introduction*. Belmont, California: Wadsworth Publishing Co. N.p. Pp. xv+450.

Since there are already in print a considerable number of introductory texts dealing with the inter-relations of logic and philosophy, some excuse is needed for adding to their number. Kahane supplies this by adhering to a well-tried formula: a compromise; his book, he notes in the Preface, is meant to fill a place between "baby" texts, which are too easy, and thin with respect to subject matter, and "high powered" texts, which are too difficult. The evaluative expressions are used with reference to the "average student", but it is not altogether clear to what sort of students the book is directed. In the British context, it seems suitable in a first-year undergraduate course for Honours Philosophy candidates, or in any undergraduate year in a subsidiary course for non-philosophers; it is not appropriate as a text in an honours or subsidiary course for Honours Mathematicians.

The development of deductive logic is confined to a consideration of classical systems, and the treatment is initially based on truth tables; thereafter, a number (18) of natural deductive rules are used to formulate proofs in sentential logic. It is difficult to discern the rationale behind this system: the set of rules is not the smallest possible; the rules are not grouped according to the sentential connectives they apply to; although the principle of using pairs of introduction and elimination rules is applied in the treatment of quantifiers, it is not used (though equally applicable) in the sentential rules; and when, at the end of the book (Ch. 20), metatheorems are discussed (but not proved), the underlying object-language system is not natural deductive at all, but axiomatic. Presumably the idea is to have to hand a goodly supply of rules for possible use, but indeed the system has little logical, intuitive, or pedagogic merit: the systems of, e.g. Fitch, Lemmon, Mates, and Leblanc, which are comparable rule-based structures, are certainly preferable on all three counts.

In the development of predicate logic, quantifiers are governed (as noted above) by pairs of introduction and elimination rules, UG and UI (universal generalisation and instantiation), and EG and EI. As is well known, these sorts of rules (especially UG and EI) have to be carefully formulated and suitably restricted to prevent invalid proofs; Kahane's treatment of this difficult task perhaps somewhat heavy-handed, though in the end commendably explicit. any writers content themselves, when setting out the rules, with saying iables must be general', or even 'the usual restrictions on variables', out giving adequate criteria; Kahane initiates the discussion in this way, g, e.g. that the move from *Fy* to (*x*)*Fx* is legitimate if *y* is "arbitrarily ted", while giving no method for ascertaining whether the criterion is met. en proceeds to consider a number of (purported) proofs, some valid and invalid, and notes correct and incorrect applications of the rules. This edure, of treatment by cases, has some merit, in that the student is given an of the sorts of situations the individual rules apply to; but then the several ctions take on a very *ad hoc* appearance, seemingly being introduced one e to deal with single difficulties. The reader is then left with the double lty of having no general principles for the imposition of restrictions, and ing no assurance that the given restrictions are just those required. Per- in the absence of any metamathematical machinery, Kahane's procedure is reasonably defensible; as noted above, the restrictions are, in the end, quite explicit.

The middle section of the book deals with traditional syllogistic logic, in A, E, I, O form, using Venn diagrams, and relations are noted between syllogistic and predicate logic. A short chapter on fallacies is followed by Part Four, Induction and Science. In this, Kahane considers some aspects of induction, probability, confirmation, explanation, and theories. The author notes that this subject matter is "highly controversial", and his treatment is in the nature of a general survey. Among the topics touched on are the ontological status of theoretical entities, Hume on causality, Mill's Methods, alternative accounts of probability (specifically, classical vs. frequency theories), the probability calculus, criteria for hypothesis acceptance, and the justification of induction. Although the material is treated in a cursory manner, its inclusion is welcome and a useful corrective to the view that a logic book must restrict itself to a consideration of classical deductive systems.

In Part Five, a number of philosophical problems, together with some logical solutions, are considered. Among these are dispositionals, counterfactuals, identity and modality, syntactic and semantic paradoxes, and synthetic *a priori* propositions. In an introductory book of this nature, complicated philosophical problems will have to receive a compressed treatment, but in addition there seems to be a number of avoidable deficiencies. Thus some questions are raised, and then left as being too complicated to be discussed; an alternative, and perhaps more appropriate procedure would be to formulate the questions in a less complicated way. Also, there is no need to couple simplicity with inaccuracy: it is stated (p. 334) that $\overline{PP} \equiv \sim\overline{PP}$ "is an explicit contradiction", which it is not, though it *may* imply one (an exception is Fitch's system); again it is said (p. 339) that sentences or propositions are about *things*, which is a misplaced (or at least controversial) use of 'things'. There is a suppressed assumption throughout this part that the application of logic to philosophy supports a particular philosophical position, namely the empiricist one (which Kahane apparently embraces, though he doesn't say so); this assumption is both mistaken and undesirable, since logic can be and should be philosophically neutral, certainly relative to general alternatives such as empiricism and rationalism.

There are a number of very useful features of the book: two appendices, a glossary, answers to the many exercises in the text, a bibliography, a list of symbols used, an index, and on the inside covers are printed the rules for the various systems considered.

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DUCASSE, C. J. (1969) *Truth, Knowledge and Causation.* London: Routledge & Kegan Paul. £2.50 (50*s*.). Pp. viii+255.

This volume contains a collection of essays written over a period of forty years. Although they have all been published (and in some cases reprinted) before, and despite some inevitably tedious repetition of detail, these essays deserve to reappear in this form if only because they are frequently dependent upon, or closely related to, one another.

As the title of this collection indicates, Professor Ducasse's concern is with a group of topics which are central to the empiricist tradition in modern philosophy. He discusses these topics in a forthright, lucid and careful manner and exhibits sensitivity to those misgivings of his real and imaginary critics which he is prepared to acknowledge.

Causation is the chief topic of the first five papers. In them Ducasse advances and defends his thesis that the causal relation is an observable relation between events. This means, it appears, that we can not only observe that an event, C, is followed by an event, E, but also observe that C is the cause of E. That is to say, evidence of temporal contiguity, though necessary, is never sufficient to establish causation. For C to be the cause of E, according to Ducasse, it is also necessary that C be the only event which takes place in the immediate environment of E. Consequently, the observational evidence that C is temporally succeeded by E and that C is the only change in the immediate environment of E constitute adequate evidence that C causes E.

It is doubtful, however, whether this analysis would impress the sceptic. For a dispute about whether C is, or is not, the cause of E, would become, according to this analysis, a dispute about whether C is, or is not, the sole change in the immediate environment of C, and it is by no means clear that observational criteria are adequate to resolve this dispute. Moreover, it could be argued that this difficulty is not, as Ducasse suggests, merely practical, on the grounds that what counts as a change, and therefore as a possible sole change, is frequently a matter of decision rather than observation. It is unfortunate that Ducasse's respect for terminological precision in other contexts failed to prevent him from recognising that no analysis of causality can hope to succeed without a careful examination of the ways in which the concepts of event, of change and of observation are employed. It is not good enough to stipulate "that nothing can, in strict propriety, ever be spoken of as a cause or an effect, except an *event*", and then to propose a definition of causality in which causes and effects are spoken of as changes.

Three essays are devoted to problems about truth and related topics. The early essay entitled 'A Liberalistic View of Truth' contains some useful comments on the claim that vagueness of meaning generates indeterminateness of truth value. The later essays, which explore certain issues in philosophical logic, are less successful. For example, Ducasse subscribes to the curious and, on the face of it, paradoxical view that the same true-or-false proposition is formulated by the sentences 'Napoleon was short' and 'Napoleon was not short'. He appears to maintain that propositions function not only as objects of beliefs but also as "contents" or constituents of beliefs. We are not told, but are left to assume, that these claims about the nature of propositions are mutually consistent.

The remaining essays are devoted to a wide variety of topics. Of these, perhaps the most valuable is Ducasse's well-known discussion of Moore's 'Refutation of Idealism'. Here, he reiterates his contention that a distinction must be drawn between connate and alien accusatives, and shows how this is relevant to Moore's criticism of the proposition that *esse* is *percipi*. Nevertheless, it is to an essay entitled 'The Method of Knowledge in Philosophy' that our attention is drawn by the preface to this collection. This is one of two concluding essays which attempt to elucidate what Ducasse considers to be the methods appropriate to philosophical investigation. The aim of philosophy, he says, consists in the formulation of accurate and precise definitions. The problem of the nature of reality, for example, is to be solved by a careful examination of the ways in which words like 'real', 'really', and 'reality' happen to be used and by the construction of a definition based upon this examination. Philosophy, like any science, is data dependent. The host of problems which are raised by such a characterisation of philosophical enquiry are neither mentioned nor discussed by Ducasse.

This book is retrospective. As such, it will be instructive to those who believe that philosophy is a non-progressive discipline. Much that Ducasse emphasised and much that was, no doubt, novel, no longer needs to be emphasised and is no longer novel. But though we might hope that the remarks Ducasse addressed to an earlier generation would still be relevant to contemporary issues, the fact is that most of what appears as valuable in these essays has since been better said by others.

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CORRECTION Causality and Statistics in Modern Physics

In Professor Dingle's article in the August issue, the expressions '*ad indefinitum*' and '*ad infinitum*' on page 230 should be interchanged. The Editor, not the author, was inadvertently responsible for the original transposition.

We have received the following announcement, and wish to associate ourselves with the deep regret it expresses:

The Department of Philosophy of the University of California, Los Angeles, deeply regrets to announce the death following a brief illness of Rudolf Carnap, Emeritus Professor of Philosophy, on 14 September 1970.

THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

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Meetings are held on Mondays in the Joint Staff Common Room at University College London beginning at 5.30 p.m. Tea is served at 5 p.m.

November 9
Mr B. Gower: Proofs: Positive and Presumptive

December 7
Dr E. Zahar: The Michelson-Morley Experiment

January 11
Dr N. Wetherick: A Psychologist Looks at Induction

February 8
Dr P. Seuren: On the Nature of Linguistic Evidence

March 15
Professor R. B. Braithwaite: A Behaviourally Testable Theory for Belief and Preference

April 19
Mr R. McGowan: Probability of Particulars

May 17
Professor C. Kilmister: Constructivity and Quantisation

June 7
Dr C. Cellucci: The Role of Church's Thesis in Intuitionistic Mathematics

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